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SOME HISTORICAL ASPECTS OF THE RECAPITULATION IDEA

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IT IS well to remember in connection with the theory of recapitulation that scientific, like other dogmas, undergo modification in the course of time. It hence seems best to speak of ideas, hypotheses, theories or doctrines rather than merely of a theory, or perchance of a law of recapitulation. Varying aspects of what we call recapitulation attracted attention at different times and appealed to or were espoused by various writers and investigators.

I have not as yet been able to learn who first used the word recapitulation in a biological sense or spoke of a recapitulation theory, but according to the *Oxford Dictionary*, the word recapitulation was used by Dyer, writing in the *Encyclopedia Britannica* III, 692/2, in 1875. In this Dictionary Lankester also is quoted (Degener, '21) as having declared in 1879 that: "Suppose . . . that the Barnacles . . . instead of recapitulating in their early life, were to develop directly from the egg to the adult form." I do not know whether the idea that a developing organism loiters along the way originated with Lankester or whether it had been entertained by others, but such a conception of recapitulation implies that much time is wasted especially by higher organisms through deviations during their ontogeny.

ARISTOTLE

In regard to recapitulation as in regard to many other matters biological, it is necessary to begin with "the master of them that know." Although I do not feel that anything Aristotle wrote necessitates this, there are those who hold a different view. Needham, for example, when referring to Aristotle's ideas of "the degree of aliveness" in embryos stated that (p. 64): "Aristotle does not here anticipate the form of the recapitulation theory, but he certainly suggests the essence of it in perfectly clear terms." Needham further stated that (pp. 69–70):

> Aristotle has a good deal to say about the theory of recapitulation, as it was afterwards to be called. He thought there was no doubt that the vegetative or nutritive soul existed in the unfertilised material of the embryo, "for nobody," as he says, "would put down the unfertilised embryo as soulless or in every sense bereft of life, (since both the semen and the embryo of an animal have every bit as much life as a plant) and it is productive up to a certain point. . . .

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As it develops it also acquires the sensitive soul in virtue of which an animal is an animal. . . . For first of all such embryos seem to live the life of a plant, and it is clear that we must be guided by this in speaking of the sensitive and the rational soul. For all three kinds of soul, not only the nutritive, must be possessed potentially before they are possessed actually."

According to Needham,

These passages show very clearly the line of thought contained in the recapitulation theory, as do the following. "For an animal does not become at the same time an animal and a man or a horse or any other particular animal," i.e. the more general appears first and the more particular later. "For the end is developed last, and the peculiar character of the species is the end of the generation in each individual," i.e. the embryo attains the point of being definitely not a plant before it attains that of being not a mollusc but a horse or a man. Aristotle concludes that the different sorts of souls enter the embryo at different stages of development, just as the shape of the embryo gradually approximates to whatever adult shape it is destined to conform to.

Needham held that (p. 75) Aristotle also ". . . foreshadowed the theory of recapitulation in his speculations on the order in which the souls came to inhabit the embryo during its growth, and in his observation that universal characteristics precede particular characteristics in embryogeny."

Meckel also spoke of Aristotle in connection with recapitulation, and referred to the following words (*De Generat. Animal.*, Book II, 3, c. 9): "For e.g. an animal does not become at the same time an animal and a man or a horse or any other particular animal. For the end is developed last, and the peculiar character of the species is the end of the generation in each individual." I acknowledge that whether or not the words quoted by Needham and referred to by Meckel justify us in regarding Aristotle as the source of the idea of recapitulation is a matter of opinion, but I do not feel that they do so, for even if Aristotle dissected fifty different kinds of animals and knew some embryonic forms, neither comparative anatomy nor embryology had developed sufficiently to afford an adequate basis for such a generalization as that of recapitulation.

HARVEY

According to Needham, the idea of recapitulation is contained also in the following words of Harvey (p. 141): "'About the fourth day the egg begin-neth to step from the life of a plant to that of an animal.' 'From that to the tenth it enjoys a sensitive and moving soul as Animals do, and after that, it is compleated by degrees and being adorned with Plumes, Bill, Clawes and other furniture, it hastens to get out.'" Needham added, however, that "These and other passages which deal with the forerunner of the theory of recapitulation, are interesting, but we have already met essentially the same idea in Aristotle."

Since Meckel also referred to Harvey, it is well to examine everything from him that seems at all pertinent to the subject. In *De Generat. Exercit.*, p. 165, we read:

. . . for all animals resemble one or other of those above mentioned [fowl, goose, duck, pigeon; frog, serpent, fish; crustacea, testacea, silkworms; sheep, goats, dogs, cats, deer, oxen; man], and agree with them either generally or specifically, and are procreated in the same manner, or the mode of their generation at least is referrible by analogy to that of one or other of them. For Nature, perfect and divine, is ever in the same things harmonious with herself, and as her works either agree or differ, (viz. in genus, species, or some other proportion), so is her agency in these (viz. generation or development) either the same or diverse.

De Generat. Exercit. p. 163: For example, before a man attains to maturity, he was a body, an infant, an embryo. And then it is indispensable to inquire further as to what he was in his mother's womb before he was an embryo or foetus. . . .

In *De Motu Cordis* (Leake transl. 1931), p. 127, Harvey declared:

> Thus divine Nature making nothing in vain, neither gives a heart to an animal where it is not needed, nor makes one before it can be used. By the same steps in the development of every animal, passing through the structural stages, I might say, of egg, worm, and fetus, it obtains perfection in each. These points are confirmed elsewhere by many observations on the formation of the fetus.

In his very interesting and instructive volume on *Early Theories of Sex Generation*, Cole also says that Harvey, in *De Motu Cordis*, has a passage which foreshadows the recapitulation doctrine in embryology, based on numerous observations of the formation of the fetus. I have already quoted the sentence from Harvey to which Cole seems to refer, and to which Meckel also referred. It is possible that Cole had a different sentence in mind and that his conclusion may hence be justified, but I have not been able to find a more pertinent passage in Harvey than the one quoted above. It is possible that Harvey had recapitulation in mind when he suggested that every animal passes through the stage of egg, worm and fetus but the general tenor of Harvey's *De Generatione* and the undeveloped state of embryology and comparative anatomy and the dormant condition of the evolution concept, scarcely justify one in concluding that he was so far in advance of his day in regard to this idea.

JOHN HUNTER

There is no doubt regarding John Hunter, however, for he expressed himself adequately even if not very clearly or fully. I do not know the exact date of the formulation of this idea in Hunter's mind, but I believe that Owen was wholly justified in speaking of "the following remarkable one [passage] from Hunter's description of his drawings illustrative of the development of the chick" of the goose. Since Hunter apparently worked on the chick as early as 1755, this passage may have been formulated about that time. It is found in the *Progress and Peculiarities of the Chick* and reads as follows (*Essays and Observations*, p. 203):

> If we were capable of following the progress of increase of the number of the parts of the most perfect animal, as they first formed in succession from the very first to its state of full perfection, we should probably be able to compare it with some one of the incomplete animals themselves, of every order of animals in the Creation, being at no stage different from some of the inferior orders. Or, in other words, if we were to take a series of animals, from the more imperfect to the perfect, we should probably find an imperfect animal, corresponding with some stage of the most perfect.

In commenting upon Hunter's words in the preface to *Observations on Certain Parts of the Animal Economy*, Owen wrote (p. 26): "We may, I think, perceive, from the evident difficulty with which Hunter expresses the idea, that his mind was oppressed with both its novelty and vastness. Men's thoughts require to be familiarized with propositions of such generality before their exact limits and full application can be appreciated." Be this as it may, it is well known that John Hunter was quite unfamiliar with the literature of the subjects with which he dealt and lacked the power of clear expression possessed by his brother William. Although Owen thought his expression rather labored, and Russell (1916) called it confused, it seems to me that it undoubtedly is a statement of the idea of recapitulation. Hunter was a man of new ideas and a comparative anatomist of very wide experience. However, since new ideas have a way of spreading independently of books, it is possible that knowledge of some version of recapitulation held by some predecessor of his may have reached him, but that is purely a matter of conjecture. Hunter's formulation of recapitulation probably remained unknown at the time because he was careless about

publication. The illustrations that accompany his study of the development of the chick have not, as far as I know, been reproduced in their entirety even to this day, and although Sedgwick in the eleventh edition of the *Britannica*, Russell in his *Form and Function*, and Garrison in his *History of Medicine*, called attention to Hunter's formulation of the theory of recapitulation, it undoubtedly will be some time before his merits will become more generally known. It is interesting that Rádl in his splendid work on the history of biological theories does not mention John Hunter.

KIELMEYER

In *Evolution and Adaptation* (p. 58), Morgan stated that Kielmeyer (1793) was the first to suggest the idea of recapitulation, and Meckel likewise mentioned Kielmeyer first. In his *Rede ueber die Verhältnisse der organischen Kräfte unter einander in der Reihe der verschiedenen Organisationen, die Geseze und Folgen dieser Verhältnisse*, Kielmeyer reverted to the idea of Aristotle that the human embryo at first has a purely vegetative life like a plant, then that of lower animals which move but do not feel, and finally reaches the stage of both motion and feeling. Carus, when speaking of Kielmeyer in his *Geschichte der Zoologie*, declared (p. 583):

"Dass der Embryo höherer Thiere die Formenzustände niederer Classen durchlaufe, hatte schon 1793 Kielmeyer ausgesprochen, dies war also nicht Oken's Verdienst. Uebrigens hat die Idee nur dann wirklich Anregendes, wenn sie bei entwickelungsgeschichtlichen Betrachtungen innerhalb der einzelnen Typen beachtet wird; ausserdem verleitet sie zu vagen Spielereien mit Analogien. . . ." (p. 593) "Bei seinen Ableitungen kommt er auch aus den Vergleich früherer Entwickelungszustände höherer Thiere mit niederen Thieren. Da ihm aber Entwickelungsgeschichte noch fern lag, gelingt es ihm nicht, diesen Satz fruchtbar zu verwenden."

This expression of recapitulation is no more definite than that made by John Hunter considerably earlier. However, that Kielmeyer considered the idea at length is evidenced by a public lecture and the publications of his students. Hunter confined himself to a brief expression.

When discussing recapitulation in his treatise on comparative anatomy, Meckel mentioned Kielmeyer first but also referred to Autenrieth (1797), Carlisle (1805), Oken (1806), Walther (1807–1808), Blumenbach (1813), Tiedemann (1819), Carus (1814 and 1818), de Blainville (1819), and himself (1806 and 1811). As far as I have been able to ascertain, none of these men except Oken made as significant an expression as John Hunter. Carlisle merely wrote (p. 5): "The punctum saliens, during its first action, is not encompassed by any fibers discoverable with microscopes, and the vascular system is not then evolved, the blood flowing forwards, and backwards, in the same vessels. The commencement of life in animals of complex structures is, from the preceding fact, like the ultimate organization of the simpler classes." The frequency with which the latter idea is mentioned by various writers suggests that it was entertained rather generally. Hertwig (1906) also mentioned Kielmeyer, Oken, Tiedemann, Carus and de Blainville but merely said that they noticed similarity in form between prenatal stages of higher and mature forms of lower animals.

Rádl called Kielmeyer an unintentional mediator between the dynamic and morphological tendencies of his time. He said he tried to explain all resemblances and differences between organisms through the effect of manifold vital "forces" such as sensibility, irritability, reproduction, secretion and propulsion and attempted to explain how all organisms, including

man, have a vegetative life at first but acquire sensibility and irritability in increasing perfection until they reach their final goal. Rádl further said that Kielmeyer tried to arrange all animals in a graded series upon this basis but that he did not get beyond generalizations. It seems that Kielmeyer held that man had passed through such a scale in the course of his development and that the geological appearance of animals agrees with the progressive series of existing animals. He apparently thought that as one passed from the higher to the lower "organizations," one sense after another dropped out, the sense of touch being the last to disappear.

Kielmeyer's address of 1793 remained difficult of access until its republication by Balss (1930). The perusal of it confirms Rádl's statement that Kielmeyer belonged to the German *Naturphilosophen*, although Kielmeyer himself in a letter to Cuvier, largely rejected their ideas. Balss held that although others, including Cuvier, a pupil of Kielmeyer's, agreed with Rádl, and some of his commentators even went so far as to call Kielmeyer the father of nature philosophy, this was due to a superficial resemblance only, and to the fact that only the above address was known to them. It seems that although Kielmeyer had partly prepared a work for publication, he withdrew it and hence the only other publication by him is an address given before a scientific society at Stuttgart in 1834.

Balss and especially Buttersack, regarded Kielmeyer as something of a genius, an estimate contained also in an inscription upon a gold medal presented to him by a scientific academy, which reads:

> Graiis Aristoteles, Harveus Britannis,
> Teutonum populis Kielmeyerus erit.

According to Balss, Kielmeyer's opinions really were based upon an extremely wide and thorough knowledge of comparative anatomy and embryology, which placed him in marked contrast to the German *Naturphilosophen*. However, Balss admitted that the address of 1793 gave one a wrong impression and called attention to theses done under Kielmeyer quoting from a volume by Münter (1840) which, it is generally recognized, was based largely if not wholly upon notes taken from Kielmeyer's lectures. Pertinent passages from this work are the following (after Balss, pp. 277–78):

> So kann man sagen, ist der Mensch anfangs fisch- und froschähnlich; er nähert sich dann mehr der Form der Cetaceen und wird endlich der Form der eigentlichen Landsäugetiere genähert. . . . Jeder organische Körper stellt mit seiner Entwicklung die Reihe der bleibenden und unter ihm stehenden Organismen auf unserer Erde dar. So z. B. stellt der Embryo einer Bohne einen Fucus in seiner bleibenden Gestalt dar; ebenso ist es der Fall mit den Fröschen, die anfangs den Fischen gleichen, dann bei weiterer Entwicklung die Form der Salamander bekommen und erst späterhin die Forschform annehmen. Kurz, der organische Körper durchgeht während seiner Entwicklung die permanente Reihe der Organismen, und umgekehrt muss man die permanente Reihe der organischen Körper an der Entwicklungsgeschichte des einzelnen Organismus studieren (S. 261).

The passages in Kielmeyer's address of 1793 which bear upon this point are the following (pp. 261–62):

> Somit wäre also nun nach sehr einfachen Gesezen dieses Verhältniss in der Reihe der Thiere abgeändert; die Simplicität dieser Geseze, die sich in eine so ungeheure Mannigfaltigkeit ergiessen, muss aber noch mehr auffallen, wenn man bedenkt, dass eben diese Geseze, nach welchen die Kräfte an die verschiedene Organisationen vertheilt sind, gerade auch die sind, nach denen die Vertheilung der Kräfte an die verschiedene Individuen der nehmlichen Gattung, ja auch an ein und dasselbe Individuum in seinen verschiedenen Entwiklungsperioden geschah: auch der Mensch und Vogel sind in ihrem ersten Zustande pflanzenartig, rege ist die Reproductionskraft in

ihnen, späterhin hebt sich in dem feuchten Elemente, in dem sie dann leben, ihre Irritabilität, auch das Herz dieser Thiere ist unzerstöhrlich reizbar, und erst späterhin schliesst sich ein Sinn nach dem andern beinahe in eben der Ordnung, wie sie in der Reihe der Organisationen von unten auf zum Vorschein kommen, in ihm auf, und was zuvor Irritabilität war, entwikelt sich am Ende zur Vorstellungsfähigkeit, oder wenigstens ihrem unsichtbaren unmittelbarsten materiellen Organ. . . .

Ja, da die Vertheilung der Kräfte in der Reihe der Organisationen dieselbe Ordnung befolgt, wie die Vertheilung in den verschiedenen Entwiklungszuständen des nehmlichen Individuums, so kann gefolgert werden, dass die Kraft durch die bei leztern die Hervorbringung geschieht, nehmlich die Reproductionskraft in ihren Gesezen mit der Kraft übereinstimme, durch die die Reihe der verschiedenen Organisationen der Erde ins Daseyn gerufen wurde, und da gerade nun auch die niedersten Klassen, bei denen die Individuen so zahlreich sind, in Gattungen am zahlreichsten hervorgebracht sind, so ist es noch mehr gestattet anzunehmen, dass die Kraft, durch die die Reihe der Gattungen hervorgebracht wurde, ihrer Natur und Gesezen nach mit der, durch die die verschiedene Entwiklungszustände bewirkt wurden, wohl einerlei sei, und wirklich liesse sich auch, wenn hier der Ort wäre, diese Idee auszuführen, zeigen, dass man durch vorsichtig aufgesuchte Analogien dahin geleitet wird, eine solche materielle Ursache zur Erklärung der Entwiklungserscheinungen anzunehmen, die man sich auch bei der ersten Hervorbringung der Organisationen auf unserer Erde wirkend vorstellen kann.

MECKEL

In his *Die Zeugung* (1805) and in his *Lehrbuch der Naturphilosophie* (1810–11), Oken says that the human embryo passes through all those stages through which other animals pass, a worm, a crustacean an insect, and so forth, in turn. This idea was elaborated somewhat by Tiedemann and was also given partial expression by Swammerdam, but a fuller discussion of it with evidence for and against it, we owe to Meckel (1811 and 1821). Meckel tried to establish proof of the idea that the developmental stages of higher animals represent the final stages of the lower, by extensive gross comparative anatomical and developmental studies. He also tried to homologize the form and function of individual organs of decidedly different groups. Had the idea of evolution gained wider adherence or more impetus, Meckel probably would have emphasized the idea of descent for he believed that development in the individual and in the group is controlled by the same forces. Meckel wrote (1821, p. 396):

Dass der Embryo höherer Thiere, ehe er seine vollkommne Ausbildung erreicht, mehrere Stufen durchläuft, wurde schon oben bemerkt; hier ist nachzuweisen, dass diese verschiednen Stufen denen entsprechen, auf welchen tiefer stehende Thiere das ganze Leben hindurch gehemmt erschienen. In der That kommt der Embryo höherer Thiere, namentlich der Säugthiere und besonders des Menschen, durch äussere Gestalt, einzelner Organe sowohl als des ganzen Körpers, Zahl, Lage, verhältnissmässige Grösse der Organe, Gewebe, Mischung und Kräfte mit unter ihm stehenden Thieren mehr oder weniger vollkommen überein.

(p. 409) Gegen diese, schon von Aristoteles, in neuerer Zeit von Harvey angedeutete, vorzüglich jetzt von mehrern Physiologen, namentlich Kielmeyer, Autenrieth, Carlisle, Oken, Walther, dem nie alternden Blumenbach, Tiedemann, Carus, Blainville und mir ihrem hohen Werthe gemäss geachtete Gleichung zwischen der Entwicklung des Embryo und der Thierreihe haben sich mehrere Schriftsteller aus verschiednen Gründen erklärt, welche hier zu untersuchen sind, sofern, im Fall daraus sich wirklich die Unrichtigkeit jener Ansicht ergäbe, das Gesetz der Identität einen bedeutenden Theil seiner Gültigkeit verlieren würde.

(p. 411) Ob der menschliche Embryo alle oder nur einige Bildungsstufen durchläuft, ist für diese Ansicht völlig gleichgültig, sobald sich nur aus sichern Thatsachen ergiebt, dass er deren mehrere, und dass er sie immer durchläuft, dass also jene Aehnlichkeiten nicht zufällig sind. . . .

Although it is true, as von Baer said, that Meckel recognized that not all stages of the lower are repeated in the development of the higher individual and although he especially considered exceptions, he nevertheless carried the idea of parallelisms to surprising lengths. He correlated various portions and organs of the same individual such as the upper and

lower halfs of the body, and extended the idea to organization and function.

Since Serres, the discoverer of Meckel's cartilage and a disciple of Saint-Hilaire, elaborated very much the same idea as did Meckel, he often is mentioned with him and the ideas they sponsored are sometimes spoken of as the "law of Meckel and Serres." This was an advance beyond the idea of parallelisms of Hunter in that it included organs and contained more of the implication of ancestry and because Meckel also considered function and abnormal development in the same light. Meckel's conclusion that abnormalities often represent arrests of development (Hemmungen) probably had its origin in his idea that lower organisms themselves represent instances of arrest of development.

CARUS

Carus, (1814) whom Meckel also mentioned, wrote as follows (p. 2):

. . . —Man erkannte, wie die ganze organische Welt nur die stufenweis erfolgende Entwickelung eines unendlichen Organismus darstelle, wie auf gleiche Weise die Entwickelungsgeschichte des thierischen Organismus in der unendlichen Menge verschiedener Thiergeschlechter sich fixire, wie der menschliche Organismus die endliche Vollendung dieser Entwickelung darstelle und wie er in den verschiedenen Perioden seiner Bildung, der Idee nach, die verschiednen Entwickelungsstufen der Thierwelt wieder durchlaufe.

Carus (p. 123) expressed doubt whether a natural system of classification could reveal the embryology of organisms but added that if it did his conceptions would not be controverted. In the 1834 edition of the *Lehrbuch der vergleichenden Zoötomie*, he expressed himself as follows:

In wiefern nun die riederen Organismen eine sehr kurze Reihe von Verwandlungen die höheren Organismen hingegen eine sehr lange Reihe derselben zu durchlaufen haben, so muss ein gewisser Parallelismus zwischen der verschiedenartigen Bildung der erstern hervortreten. Dieser Parallelismus ist es welcher verursacht dass die einzelnen Entwickelungsstufen eines höheren Organismus stets an eine bestimmte Art der tieferen Bildungen, nicht sowohl als ein diesen Vollkommenen homogens sich anschliessen, sondern an das Wesentliche derselben erinnern.

In commenting upon this statement Carus said that this is why in the earliest stages of development, man has white blood only, as is the case in mollusks, and that like these he has no skeleton at that time.

VON BAER

I do not know who was originally or primarily responsible for the spread of the idea that von Baer formulated the "law" of recapitulation or for the fact that various writers regard von Baer's "law" and the theory of recapitulation as synonymous. According to the *Oxford Dictionary* Salesby, 1903, writing in *Academy* declared, "von Baer's law may be stated thus: Ontogeny is the recapitulation of phylogeny." Even prominent biologists are confused regarding this matter. Kellogg, for example, wrote in 1924: "This recapitulation theory is one of the greatest generalizations that has been made in biological study. It was first formulated by Karl von Baer and later made more specific—too specific, indeed—by Haeckel. . . ." Wilson, while discussing recapitulation in a lecture at Woods Holl in 1894, referred to "a leading English biologist," who declared during that year "that von Baer's law falls to the ground." These words seem to imply that either the English biologist to whom Wilson referred, or the latter himself, or possibly both, also regarded the theory of recapitulation and von Baer's "law" as identical. Similar confusion is found in the words of Woodruff (1923) that von Baer's "emphasis on the resemblance of certain embryonic stages of higher animals to adult stages of lower forms were crystallized by his suc-

cessors, under the influence of the evolution theory as the germ layer theory and the recapitulation theory."

Although Needham had attributed the origin of the theory to Aristotle and Harvey he nevertheless spoke of ". . . the recapitulation theory, which was first clearly formulated by von Baer . . .," and repeated this statement several times. On page 1629, under the caption, "The Theory of Recapitulation," Needham summarized as follows:

> The idea originated in the early years of the nineteenth century with Meckel; Serres; the elder Agassiz and von Baer. In those days the statement made was simply that the embryos of all species were more alike than the adults, and that the younger you took the embryos the more alike they were, so that after the advent of the cell theory, it was affirmed that when you got back to the zygote, the egg-cells of dog and duck were morphologically indistinguishable. Or as Aristotle had said, the more general characters appeared as a rule in ontogeny before the more specific ones. The following passage from von Baer, already well known, became still more famous by being quoted by Darwin in the *Origin of Species:* "The embryos of mammals, of birds, lizards, and snakes, probably also of chelonia, are in their earliest states exceedingly like one another, both as a whole, and in the mode of development of their parts; so much so, in fact, that we can often only distinguish the embryos by their size. In my possession are two little embryos in spirit, whose names I have omitted to attach, and at present I am quite unable to say to what class they belong. They may be lizards or small birds, or very young mammals, so complete is the similarity in the mode of formation of the head and trunk in these animals. The extremities, however, are still absent in these embryos. But even if they had existed in the earliest stage of their development, we should learn nothing, for the feet of lizards and mammals, the wings and feet of birds, no less than the hands and feet of man, all arise from the same fundamental form." This statement of the case was fully accepted by Darwin himself and by all the exponents of evolution. "Community in embryonic structure," he said "reveals community in descent." This was not affected by cases where the relation did not hold, dissimilarity of development did not prove discommunity of descent, because developmental stages might be missed out or so modified by the requirements of embryonic life as not to be recognisable. This process was called 'caenogenetic modification' by Haeckel.

Morgan (1903) and Montgomery (1906) correctly stated that von Baer opposed the idea of recapitulation and Sewertzoff (1928) says he opposed that of Meckel-Serres and of Müller-Haeckel as well. That von Baer cannot rightly be regarded as the author of the theory of recapitulation is shown clearly by his own words (p. 220):

> Dadurch ist aber noch nicht erwiesen, dass jeder Embryo einer höhern Thierform allmählig die niedern Thierformen durchlaufe. Vielmehr scheint sich der Typus jedes Thiers gleich anfangs im Embryo zu fixiren und die ganze Entwickelung zu beherrschen. . . . *Der Embryo des Wirbelthiers ist schon anfangs ein Wirbelthier. . . . Mithin durchlaufen die Embryonen der Wirbelthiere in ihrer Entwickelung gar keine (bekannten) bleibenden Thierformen.*
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> (p. 222) Diese Bemerkungen führen uns auf die Frage, ob wir denn nicht immer weiter zurückgehend auf eine Stufe gelangen können, wo auch die Embryonen der Wirbelthiere und der Wirbellosen übereinstimmen. Ich werde in einem spätern Zusatze, wo besonders von der Verschiedenheit des Bildungsschema für die Haupttypen der Thiere gesprochen wird, zu erweisen suchen, dass auch die gegliederte Thierreihe mit einem Primitivstreifen ihre Entwickelung beginnt. In diesem kurzen Momente würde also Uebereinstimmung zwischen ihnen und den Wirbelthieren seyn. In dem eigentlichen Keimzustande ist aber wahrscheinlich Uebereinstimmung unter allen Embryonen, die aus einem wahren Ei sich entwickeln. Hierin liegt ein wesentlicher Grund, den Keim für das Thier selbst anzusehen (Schol. II.) Wenn im Keime des Vogels der Primitivstreifen sich bildet, so sind wir zwar geneigt zu sagen: jetzt fängt der Embryo an.
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> (p. 224) Hiermit Stimmt es ganz, dass die Blasenform die allgemeine Urform ist; denn was wäre allen Thieren mehr gemeinsam, als der Gegensatz einer innern und äussern Fläche?
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> 2) *Aus dem Allgemeinsten der Formverhältnisse bildet sich das weniger Allgemeine und so fort, bis endlich das Speciellste auftritt.*

In Scholion V as given in volume I, page 199, of von Baer's *Entwickelungsgeschichte*, he wrote: "Ueber das Verhältniss der Formen, die das Individuum in

den verschiedenen Stufen seiner Entwickelung annimmt." Von Baer says that he is giving special consideration to the doctrine of recapitulation because of its importance and because the question had recently received much attention. Since the first volume of von Baer's work was published in 1828, these words must hence antedate that year.

Von Baer said further that this idea became current or active, literally alive (lebendig) at a time when no connected investigations on early development were known except those of Malpighi and Wolff, and that the idea was "vorzüglich durchgeführt," by a man who probably possessed the largest knowledge concerning development of higher organisms, and that this could not help but gain many adherents for it. He added that the theory in all its implications, was not accepted unreservedly by the more thoughtful individuals and especially not by him whose name had gained it most acceptance. He unfortunately did not mention his person by name, but it is very probable that he was referring to Meckel. This was also the opinion of Hertwig (1906) who, when quoting the above words from von Baer, inserted Meckel's name in the proper place.

The conclusion which I have reached regarding von Baer was reached also by Keibel who, after stating the view of Meckel on recapitulation, declared (p. 724): "Ganz anders sieht Baer die Dinge an. In bewusstem Gegensatze zur Lehre Meckels, welche man als die herrschende Lehre jener Tage ansehen darf, sagt er: 'Die individuelle Entwickelung der höheren Tierformen durchläuft nicht die ausgebildeten Formen niederer Tiere.' "

Von Baer spoke of the idea of recapitulation current in his day as follows (p. 199):

Wenige Darstellungen von Verhältnissen in der organischen Welt haben so viel Beifall gefunden, als die: dass die höheren Thierformen in den einzelnen Stufen der Entwickelung des Individuums vom ersten Entstehen an bis zur erlangten Ausbildung den bleibenden Formen in der Thierreihe entsprechen, und dass die Entwickelung der einzelnen Thiere nach denselben Gesetzen, wie die der ganzen Thierreihe, erfolge, das höher organisirte Thier also in seiner individuellen Ausbildung dem Wesentlichen nach die unter ihm stehenden, bleibenden Stufen durchläuft, so dass die periodischen Verschiedenheiten des Individuums sich auf die Verschiedenheiten der bleibenden Thierformen zurückführen lassen. (The original is in italics.)

This statement by von Baer certainly shows that the idea of recapitulation current in his day included the idea of descent; that ontogeny repeats phylogeny.

Von Baer says that doubts arose in his own mind regarding the theory because he did not find that the differences among living animals would permit one to arrange them in one progressive series as should be possible if the development of the individual is to be regarded as repeating that of the series. He then raised a number of objections to it under the heading "Zweifel und Einwürfe" and declared that one cannot hold that an embryo retraces an entire "Thierreihe" because it does not pass from one type of form into the other. He concluded further that the development of an individual organ is determined by two circumstances; one, a progressive differentiation, "Ausbildung," of the animal body, and two, a "Fortbildung," or progression from a more generalized into a more specialized form, and then emphasized that the embryos of higher forms were not like the adult, but more like the embryonic forms of lower animals. These ideas often have been spoken of as the law of divergence and of embryonic resemblance.

In an important contribution upon the relation of ontogeny to phylogeny Sewertzoff (1928, p. 152) summarizes von Baer's ideas as follows:

. . . the order of appearance of the features characteristic of a mature animal during its ontogeny corresponds with the order of appearance of these features in the phylogeny of its ancestors. . . . The law of von Baer shows us in what order the characters present in a recent mature animal appeared in its ancestors; the law of recapitulation, on the contrary, shows the order of appearance of the features which once characterized the mature progenitors, but which are replaced by others in recent forms. . . . We see that both laws are important for the investigation of phylogeny: the one (recapitulation) enables us to reconstruct the nature (Bau) and the order of appearance of those ancestral characters which have not survived in recent forms; the other (law of von Baer) enables us to determine the order of appearance of those characters which have survived in recent forms. (Author's translation.)

As Minot well said, "Von Baer, the creator of modern scientific embryology, called attention in 1828 to the limitations which must necessarily be put upon Meckel's generalization. It is to be regretted that von Baer's wise thought on this subject has not been more appreciated." (p.30)

Since as Bacon said truth is more likely to arise from error than from confusion, it is well to add that when von Baer stated that the embryos of higher animals passed through embryonic stages of the lower and that the general characters peculiar to the large groups appeared first and the more restricted later on in ontogeny, he was, to be sure, expressing an idea of recapitulation. To this extent then von Baer and his admirer Johannes Müller, also were recapitulationists. However, since von Baer expressed himself very clearly, there is no justification for saying that the theory of recapitulation was formulated by him, for he very clearly rejected the theory as formulated before his time and made no claim to authorship of another.

Hertwig (1906) correctly summarized the views of von Baer as follows (p. 43):

Endlich haben wir noch näher auf die Stellung einzugehen, welche BAER eggenüber der Lehre vom Parallelismus zwischen der individuellen Metamorphose und der Metamorphose des Tierreichs einnimmt. Er hält die namentlich von MECKEL ausgebildete Ansicht, dass der Embryo höherer Tiere die bleibenden Formen der niederen Tiere, durchlaufe, für nicht berechtigt und sucht dagegen den Parallelismus in folgender Weise zu erklären; Den Erklärungsgrund findet er darin, dass sich jedes Tier durch Umwandlung aus einer allgemeinen in eine sich immer mehr specificierende besondere Form entwickelt. "Daher ist es notwendig, dass wir in der einen wirklich historisch begründeten Folge und in der anderen genetisch gedachten Reihe eine Uebereinstimmung der in dieser fortgehenden inneren Sonderung finden, dass sich überhaupt eine Menge Uebereinstimmungen zwischen dem Embryo höherer Tiere und der bleibenden Form niederer Tiere nachweisen lassen" (1828, p. 220). "Anstatt die anderen bestimmten Formen zu durchlaufen, scheidet sich vielmehr, jeder Embryo einer bestimmten Tierform von ihnen. Im Grunde ist also nie der Embryo einer höheren Tierform einer anderen Tierform gleich, sondern nur ihrem Embryo. Nur dadurch, dass die am wenigsten ausgebildeten Tierformen vom Embryonenzustand sich wenig entfernen, behalten sie einige Aehnlichkeit mit den Embryonen höherer Tierformen. Diese Aehnlichkeit ist also, wenn unsere Darstellung gegründet ist, auf keine Weise das Bedingende der Entwickelungsgeschichte höherer Tiere, sondern nur eine Folge der Organisation der niederen" (p. 224). "Der Embryo geht nie durch eine andere Tierform hindurch, sondern nur durch den Indifferenzzustand zwischen seiner Form und einer anderen" (p. 230). "Mithin durchlaufen die Embryonen der Wirbeltiere in ihrer Entwickelung gar keine bekannten bleibenden Tierformen" (p. 220).

In spite of this correct summary of the views of von Baer, Hertwig, in the same article, strangely enough wrote (p. 37): "Die Lehre von der Parallele zwischen der 'individuellen Metamorphose' und 'der Metamorphose des Tierreiches' (C. E. v. BAER, 1828, p. 201) war am Anfang unseres Jahrhunderts unter Anatomen und Physiologen weit verbreitet. Als Gewährsmann hierfür sei C. E. v. BAER citiert (1828, p. 199)." The previous quotations from von Baer show clearly that he held views directly opposite to those attributed to him here by Hertwig and that it is wrong to call him a guar-

antor "Gewährsmann" of the idea of parallelism except in a very restricted sense. In a discussion which bears the marginal notation "Folgerungen, die man hierauf gebaut hat," von Baer says (pp. 200–201):

> Eine unvermeidliche Folge jener als Naturgesetz betrachteten Vorstellungsweise war die, dass eine früher herrschende, seitdem ziemlich allgemein als unbegründet betrachtete Ansicht von der einreihigen Stufenfolge der verschiedenen Thierformen allmählig wieder festern Fuss gewann, und wenn auch oft nicht deutlich ausgesprochen, ja selbst ohne Bewusstseyn der Forscher bei Urtheilen über thierische Formen in Anwendung kam. Auch muss man gestehen, dass, wenn jenes Naturgesetz angenommen wurde, die Consequenz ebenfalls die Aufnahme dieser Ansicht forderte. Man hatte dann nur *Einen* Weg der Metamorphose, den der fernern Ausbildung, entweder erreicht in Einem Individuum (*die individuelle Metamorphose*), oder durch die verschiedenen Thierformen (*die Metamorphose des Thierreiches*), und die Krankheit durfte man geradezu eine *rückschreitende Metamorphose* nennen, weil eine einreihige Metamorphose wie eine Eisenbahn nur vorwärts oder rückwärts gehen lässt, nicht zur Seite.

It was a real surprise to find that in the historical introduction to the *Handbuch der Entwickelungslehre der Wirbeltiere*, from which the above quotations from Hertwig were taken he quoted a sentence, only the last portion of which I was able to find in von Baer. This sentence reads a follows (p. 43): " 'Da der Keim das unausgebildete Tier selbst ist, so kann man nicht ohne Grund behaupten, dass die einfache Blasenform die gemeinschaftliche Grundform ist, aus der sich alle Tiere nicht nur der Idee nach, sondern historisch entwickeln.' " That Hertwig recognized that these words seem to contradict von Baer is shown by his introductory statement that they were not necessarily contradictory of other conceptions held by von Baer. Hertwig wrote (p. 43): "Zu diesem Gedankegang [That contained in the previous quotation] ist es als kein Widerspruch zu betrachten, wenn in demselben Scholion BAER auf die Frage, ob nicht im Beginne der Entwickelung alle Tiere sich im wesentlichen gleich sind und ob nicht für alle eine gemeinsame Urform besteht, die öfters citierte Antwort giebt (p. 224)." (Here follows the above sentence.) Since the word of Hertwig carried great weight and since the comprehensive *Handbuch* of which he was editor had a very wide distribution, one is prompted to believe that he was largely responsible for widespread misconceptions regarding von Baer and the theory of recapitulation.

FRITZ MÜLLER

The confusion that exists regarding von Baer is duplicated in the case of Fritz Müller. Locy (1908), who gave a good statement regarding the theory of recapitulation, declared that: "It was suggested in the writings of von Baer and Louis Agassiz, but received its first clear complete expression in 1863 in the writings of Fritz Müller. . . . It received its most sweeping application in the work of Ernest Haeckel." In his recent text-book on embryology, Jordan (1926), while writing upon recapitulation, likewise wrote: ". . . but it was first clearly formulated by Fritz Müller in 1863, and received its most sweeping application in the works of Haeckel (1866)." Arey, in a similar text, wrote to the same effect in 1931, saying: "This law of recapitulation was first stated clearly by Müller in 1863, and was termed by Haeckel, the law of biogenesis." Since none of these three authors referred to a particular publication of Müller's, it is possible that they depended upon the statement of Haeckel and certain British writers. I think it probable that Locy depended upon the words of Haeckel for his information and that others may have depended upon

Locy, who was specially interested in the history of biology.

In a translation of Haeckel's *Die Natürliche Schöpfungsgeschichte* by Lankester, it is stated that: "When Darwin, in 1859, at last accomplished this, he also in his fourteenth chapter of his chief work, briefly referred to the great importance of embryonic evidence. Still Fritz Müller was the first to discuss the subject fully and clearly, which he did in connection with crustacea in his admirable work, *Für Darwin*." Marshall (1894) likewise wrote: ". . . such is the recapitulation theory, hinted at by Agassiz, and suggested more directly in the writings of von Baer, but first clearly enunciated by Fritz Müller, and since elaborated by many, notably by Balfour and by Ernst Haeckel. . . . It was the elder Agassiz who first directed attention to the remarkable agreement between the embryonic growth and their paleontological history." A little farther on, Marshall spoke of ". . . the clear enunciation of the recapitulation theory of Fritz Müller."

It is highly significant, it seems to me, that Müller made no claim to authorship of the recapitulation theory and it is of special interest that he rejected the conceptions on recapitulations first brought to his attention by his "unforgettable teacher," Johannes Müller. These were those of von Baer, which he said he had entertained for many years, "lange Jahre." Fritz Müller also controverted the statement of Agassiz, of whom and of Johannes Müller he nevertheless spoke as the most trustworthy or authoritative masters, "bewähresten Meister."

Fritz Müller's essay entitled *Für Darwin* was dated Destero, September 7, 1863, but it was not published by Engelmann, of Leipzig, until 1864. I mention these facts merely because sometimes the one and sometimes the other year is given for this article. In subdivision ten of this essay, as found on pages 242–248 of Müller's *Werke, Briefe und Leben*, edited by Möller, is stated that one can scarcely speak of a typical manner of development of crustacea through individual groups such as orders, families, etc., and Müller emphasized that this should be possible if, as according to the teachings of the schools, the type of the organism is revealed most clearly during its development. He then quoted from Johannes Müller and Agassiz regarding their ideas on this matter. Fritz Müller's comment upon these quotations suggests that he no longer held the views of his revered teacher Johannes, to the effect that embryology can serve as a control in the classification of organisms, because as he said, the idea of development implies not merely an increase in size but also a progressing differentiation from which it follows that the earlier the stage of development of an organ, the more it resembles the type, and that the types of comparative anatomy and embryology should hence agree.

In commenting upon a quotation from Johannes, Fritz correctly stated that the former attacked the idea that a progressive series, "Stufenleiter," or scale in the sense of Aristotle, exists among animals and that they pass through a number of "Thierstufen" during development. He then quoted further from his namesake to the effect that the truth in these ideas lies in the fact that an embryo in the early stages of development bears evidence of the "Abtheilung" from which only the type of its class, order, etc., develops later on. Fritz unfortunately did not give the source of this quotation from Johannes but the ideas were expressed previously by von Baer in connection with the discussion of the subject of recapitula-

tion in his *Entwickelungsgeschichte*. In his *Physiologie des Menschen* Johannes wrote as follows (p. 716):

Am Schlusse dieser Uebersicht über die allgemeinsten Entwickelungen im Ei des Menschen ist die schon gelegentlich berührte Frage ausführlicher zu erörtern, wie die Aehnlichkeiten, welche die Embryonen in den verschiedenen Classen mit einander darbieten, anzusehen sind. Es ist noch nicht sehr lange her, dass mehrere Naturforscher die Idee aufstellten und ganz ernstlich nahmen, dass der Menschenfötus die niederen Thierstufen bis zum Menschen durchlaufe, und dass er in den verschiedenen Perioden des Fötuslebens verschiedenen Thierstufen gleiche. In dieser Weise ausgedrückt klingt jene Aehnlichkeit sehr abentheuerlich und findet auch in keiner Weise statt, wie v. BAER sehr schön gezeigt hat. Denn der Embryo gleicht in der That nie weder einem Strahlenthier, noch einem Insect, noch einem Molluscum, noch einem Wurme. Die Formationspläne dieser Thiere sind eben himmelweit von dem der Wirbelthiere verschieden. Der Mensch konnte daher höchstens seinen Verwandten ähnlich seyn, nämlich den Wirbelthieren, da er selbst Wirbelthier ist und mit ihnen den allgemeinen Plan der Wirbelthiere theilt. Er gleicht aber, auch nicht zu gewisser Zeit einem Fisch, zu anderer einem Amphibium, Vogel u.s.w.; sondern er gleicht einem Fisch gerade so weit er einem Vogel und Amphibium gleicht, oder wie fern alle diese Wirbelthiere sind. Anfangs tragen aber die Embryonen aller Wirbelthiere das Gemeinsame und Einfachste vom Typus eines Wirbelthiers am reinsten an sich, und daher gleichen sich die Embryonen aller Wirbelthiere in der ersten Zeit so sehr, dass es oft schwer ist, sie von einander zu unterscheiden. Fisch, Amphibium, Vogel, Säugethier und Mensch stehen also anfangs dem einfachsten gemeinsamen Typus am nächsten, und entfernen sich allmählig davon, so dass die Extremität anfangs gleich, die Bestimmung zu Flosse, Flügel, Fuss, Hand u.s.w. erhält. Deswegen haben alle Embryonen anfangs Bogen am Halse und Spalten dazwischen, die uneigentlich sogenannten Kiemenbogen, ein Ausdruck des allgemeinen Plans, an welchem in der That noch nichts von einer Kieme ist. In diesen Bogen verlaufen bei allen Thieren Aortenbogen, die sich hinten zur Aorta wieder vereinigen. Nur bei den Fischen entsteht hier eine progressive Metamorphose, indem sich an einigen dieser Bogen Kiemenblättchen bilden und die Gefässbogen sich in ein System von Gefässfedern mit arteriösen und venösen Stämmen verwandeln, welche letztere dann erst wieder die Aorta zusammensetzen. Bei den nackten Amphibien geschieht das auch, aber ihre Kiemen vergehen bei der Verwandlung, ihre Kiemengefässe werden wieder auf die ursprünglichen unverzweigten Bogen reducirt und ihre Kiemenbogen gehen grösstentheils ein, wie sie bei den beschuppten Amphibien, Vögeln, Säugethieren und dem Menschen überhaupt sogleich in andere perennirende Bildungen verwandelt werden. Hier gehen auch die mehrfachen Aortenbogen, Ausdruck des allgemeinsten und einfachsten Plans der Wirbelthiere, ein, und es bleiben bei den beschuppten Amphibien nur entweder 4 oder 2, bei den Vögeln, Säugethieren und dem Menschen nur einer übrig.

The second master to whom Fritz Müller referred in his criticism of recapitulation in *Für Darwin* was Louis Agassiz. He quoted the latter's *Principles of Zoology*, Part I, revised edition, Boston, 1856. These quotations are to the effect that the ovarian ova of all animals are "identical" small cells with yolk, nucleus, and nucleoli, and that the embryos of different animals resemble each other the more the younger they are. Müller added that he did not know whether any one today (1863) would be inclined to subscribe fully to such views, for all ova are alike only as all insects are alike in that they have a head, chest and abdomen, and provided one compares only what they have in common! He further indicated his dissent from Agassiz's conception of recapitulation and also took up some other statements contained in the quotation from the latter, which he said are wholly untenable.

It is significant that in *Für Darwin*, Fritz Müller especially considered but rejected, the very thing that his "unforgettable" teacher, Johannes, had regarded as the sole truth in the idea of recapitulation, namely, that "Embryonen, Jugendzustände verschiedener Thiere gleichen einander um so mehr, je jünger sie sind, oder wie Johannes Müller es ausdrückt, nähern sich um so mehr dem gemeinsamen Typus." Fritz Müller pointed out that there are many exceptions to this idea

which is frequently and rightly spoken of as von Baer's law, and that these exceptions occur in very closely related organisms in which the opposite is true. He then turned to the development of crustacea and declared that a study of them gives a truthful picture of the transformations through which the species passed in the course of countless thousands of years up to its present stage of development, and this of course was recapitulation, with emphasis upon its historical aspect. However, Müller made no special formulation of his recapitulation idea but brought proof in support of a certain aspect of it and especially of evolution. Others had already contributed the phylogenetic and paleontologic aspects of the theory. This was recognized by Thomson (1917) who wrote: "In Fritz Müller's *Facts for Darwin*, the recapitulation idea was applied in detail to Crustaceans. . . ."

As the title of *Für Darwin* suggests, Müller's main conclusion was that his studies on the development of crustacea confirmed Darwin's idea of evolution. This is evident especially from the concluding paragraph in this essay, which reads as follows:

> Manches in den obigen Aufstellungen mag verfehlt sein, manche Deutung misslungen, manche Thatsache nicht ins rechte Licht gestellt. Eines aber, hoffe ich, soll mir gelungen sein,—unbefangene Leser zu überzeugen, dass wirklich Darwin's Lehre, wie für so viele andere ohne sie unerklärbare Thatsachen, so auch für die Entwicklungsgeschichte der Kruster den Schlüssel des Verständnisses bietet. Die Mängel also dieses Versuches wolle man nicht dem von der sicheren Hand des Meisters vorgezeichneten Plane, man wolle sie einzig dem Ungeschick des Handlangers zur Last legen, der nicht für jedes Werkstück die rechte Stelle zu finden verstand. (Fritz Müller, *Werke, Briefe und Leben*, volume I, page 259.)

Müller's aim and conclusion hence very clearly were not the formulation of a theory of recapitulation but to bring support to Darwin. This was recognized also by the English translators of *Für Darwin*, who rendered the title as *Facts and Arguments for Darwin*. Fritz Müller emphasized that the idea that mature forms of lower organisms represent developmental stages of the higher is illustrated especially well in the case of crustacea. He declared that he observed the development of these forms, "aus dem Gesichtspuncte der Darwin'schen Lehre," and emphasized that the story of their development often is "verwischt" because it takes a direct course to its goal and that the record also may be "gefälscht" by the struggle for existence. The last conception later was called caenogenesis by Haeckel.

My conclusion regarding the significance of Müller's *Für Darwin* is that of Nordenskiöld, who wrote: "Having from the very beginning been entirely won over to Darwin's theory, he resolved to prove it by applying it in detail to a suitable animal group, for which purpose he chose Crustacea, which in his adopted country exist in a multitude of forms." That Haeckel also recognized this as the real significance of Müller's study is indicated by his statement that Müller attempted to demonstrate the value of the theory of evolution in the study of the development of crustacea. In *Die Welträthsel*, Haeckel wrote: "Schon Darwin hatte (1859) die grosse Bedeutung seiner Theorie für die Erklärung der Embryologie betont, und Fritz Müller hatte dieselbe (1864) an dem Beispiele einer einzelnen Thierklasse, den Krustaceen, nachzuweisen versucht, in der geistvollen kleinen Schrift: 'Für Darwin' (1864)." (p. 94)

In his discussion of *Das biogenetische Grundgesetz und die Cenogenese*, Keibel (1897) declared (p. 725):

> Der erste, der von dem Darwinschen Werke ausgehend die Entwickelungsgeschichte mit der Descendenztheorie in den engsten Zusammenhang brachte, war

Fr. Müller. Nach Fr. Müller ist die Entwickelungsgeschichte des Individuums als eine kurze und vereinfachte Wiederholung, gewissermassen als eine Rekapitulation des Entwickelungsganges der Arten aufzufassen. Müllers Untersuchung, die in seiner Arbeit 'Für Darwin' niedergelegt wurde, erschien 1864. Müller fusst wesentlich auf Untersuchungen an Krustern. Ich stelle hier einige der wichtigsten Sätze zusammen, zu denen er durch diese Untersuchungen geführt wurde.

These words of Keibel's, which were followed by quotations from Müller's article *Für Darwin* may also have been misinterpreted by those who did not read them critically. On the basis of his studies of crustacea, Müller considered the extent to which recapitulation might occur and how it might be modified.

AGASSIZ

Although Agassiz apparently was concerned with establishing other things than recapitulation, in his monograph on classification of 1857, the idea of phylogenetic recapitulation nevertheless is suggested in the following paragraphs from this work.

The parallelism between the order of succession of animals in geological times, and the changes their living representatives undergo during their embryological growth, exhibits thought. . . . (p. 134)

The parallelism between the gradation among animals and the changes they undergo during their growth, exhibits thought. . . . (p. 135)

Animals are linked as closely by their mode of development, by their relative standing in their respective classes, by the order in which they have made their appearance upon earth. . . . (p. 136)

In commenting upon these statements in a foot-note, Russell pertinently said (p. 203) that Agassiz "considered the classificatory categories to be the categories of the Creator's thought and hence natural, and in no sense mere conventions," and as Morgan pointed out, since Agassiz was not an evolutionist, his idea of recapitulation was "without the idea of ancestry which the theory of evolution implied." Morgan further stated that Agassiz, in the Lowell lectures of 1848, proposed "for the first time the theory that the embryo of the higher forms resemble not so much lower adult animals living at the present time, as those that lived in past times." In a foot-note, Morgan added that "Carl Vogt in 1842 suggested that fossil species, in their historical succession, pass through changes similar to those which the embryos of living forms undergo." Montgomery also referred to Vogt (1851) in connection with his discussion of recapitulation.

DARWIN

Since G. B. T. (whose identity the publishers could not ascertain) in the introduction to *A Journal of Researches*, by Charles Darwin, says that: "It would be useless to attempt to expound the doctrines of . . . the facts of the stages through which embryos of animals pass, resembling simpler animals or the early stages of widely different animals," it is well to inquire regarding Darwin's ideas concerning recapitulation. In *The Origin of Species*, Darwin, when quoting approvingly from von Baer, wrote (pp. 387–88):

So again it has been shown that generally the embryos of the most distinct species belonging to the same class are closely similar, but become, when fully developed, widely dissimilar. . . . A trace of the law of embryonic resemblance occasionally lasts till a rather late age. . . . (p. 396) Thus, community in embryonic structure reveals community of descent; but dissimilarity in embryonic development does not prove discommunity of descent, for in one of two groups the developmental stages may have been suppressed, or may have been so greatly modified through adaptation to new habits of life, as to be no longer recognisable. . . . As the embryo often shows us more or less plainly the structure of the less modified and ancient progenitor of the group, we can see why ancient and extinct forms so often resemble in their adult state the embryos of existing species of the same class.

It will be recalled that Darwin himself published several volumes on fossil and living barnacles and acorn shells during 1851 and 1854. He hence was especially interested in the work of Fritz Müller on the same group, and it is noteworthy that he apparently did not regard the latter as the author of the theory of recapitulation and that he also correctly understood the position of von Baer.

Vogt, Tiedemann and Agassiz called attention to the paleontological evidence and Fritz Müller, while rejecting the idea of recapitulation of Johannes Müller and Agassiz and by inference also that of von Baer, found that recapitulation in modified form is particularly well-illustrated in the development of certain crustacea. He did not at all claim to have formulated a theory of recapitulation and considered recapitulation in the light of evolution. This meant stress on descent. Müller also declared that the ontogenetic record is modified by the struggle for existence, "Kampf ums Dasein," during development, and also by abbreviation of the latter in consequence of a tendency on the part of the organism to take a short cut.

Haeckel's formulation made recapitulation into a slogan but this could not make it a law, though his enthusiastic advocacy of it stimulated much work in embryology. It was he especially who regarded it as a law and defended it in a very outspoken way. However, not even Haeckel went so far as did Thomson (1917) when he declared that: ". . . there is an ever-growing mass of facts relating to peculiar psychoses in child and adult which we must recognize as vestigial and recapitulatory." But this raises the question of the validity of the law and with that I am not concerned here. Any reader desiring information on this aspect of the subject will find the discussions of Cumings, Cunningham, Davidson, Gaskell, Garstang, Lankester, Montgomery, Morgan, Russell, Sedgwick, Sewertzoff and Shumway of special interest not only for the subject matter but for the references to the literature which they contain. For the information of readers I will also add that this discussion was given as an address before the Western Society of American Naturalists in December, 1932, and that this fact very largely determined its form and content. Articles which have appeared since that time are hence not considered except that I should direct attention to the fact that Zirkle in a very recent historical review of the idea of the inheritance of acquired characters, declared that Haeckel is "the inventor of the biogenetic law" and accused him of "intellectual inertia"!

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MORPHOGENESIS OF THE SHOULDER ARCHITECTURE

PART III. AMPHIBIA

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IN A previous contribution to this series of papers the author (1933) discussed briefly the general situation obtaining in the region of the shoulder girdle in fishes. In the present part Amphibia will be considered. In this class there are similar problems encountered, in that living representatives have had just as long a time to develop tangential specializations since they diverged from the common tetrapod ancestor as have the higher vertebrates; and fossil remains, although of help in some respects, are misleading in others.

In the class Pisces the morphological trend was largely toward the development of a fin useful in swimming. In the terrestrial Amphibia, on the contrary, the objective was towards limbs suitable for walking. It is not at all improbable that the change from fin to limb form proved to be a less difficult matter for the vertebrates than the development of the mechanism within the central nervous system whereby the body was held above the ground while steps were being taken. This—the acquisition of posture—must have had the most profound effect in accelerating or retarding different lines of vertebrate evolution, and yet it has been universally ignored by morphologists. An inability to develop a higher type of nervous posture may well have prevented the ancestors of modern amphibians from joining the tetrapod line of ascent, condemning them to the relinquishment of the terrestrial objective.

CROSSOPTERYGIAN STAGE

It was stated in the section on Pisces that the significance of rhipidistian crossopterygian fossils could be discussed better with amphibians than with fish. It will also be necessary to mention certain myological and neurological details of *Polypterus* in this connection.

There appears to be ample reason for considering that the skeletal details of Devonian crossopterygians fulfilled the basic requirements necessary for the tetrapod ancestry. In the shoulder region they had a membranous girdle consisting of supracleithrum above, a large cleithrum, and an anteroventrally directed clavicle. Presumably also there was connection to the skull through a post-temporal. Details in *Sauripterus* and *Eusthenopteron* appear to have been essentially similar, but are indicated with more precision in known remains of the latter, and so that form is used for illustration (fig. 1). The base of the small scapulocoracoid was bifurcated in *Eusthenopteron*, but seems to have been relatively larger in *Sauripterus* with the possibility that in that genus it may have been more completely separable into distinct parts.

Of the more distal elements there is good material representing both genera, and these agree in plan of elements constituting humerus, radius, and a considerably longer ulna. It can hardly be doubted that in antibrachial details they follow a plan that was truly ancestral.

In an attempt to interpret the basic carpal plan one may build a plausible story around almost any attractive thesis, and *Eusthenopteron* may be so employed. This crossopt offers one difficulty, however, which appears to be almost insurmountable, in the fact that its endoskeletal elements were so few (fig. 2). These are clearly shown in the illustrations of Bryder elevated and postaxial depressed. In spite of this fact it has been uniformly considered that the preaxial border was the one depressed in Devonian crossopts, until Romer and Byrne (1931) pointed out the error. Hence the ulna is the more ventral of the antibrachial bones, and it is much longer in *Sauripterus* than the radius. Other significant details consist

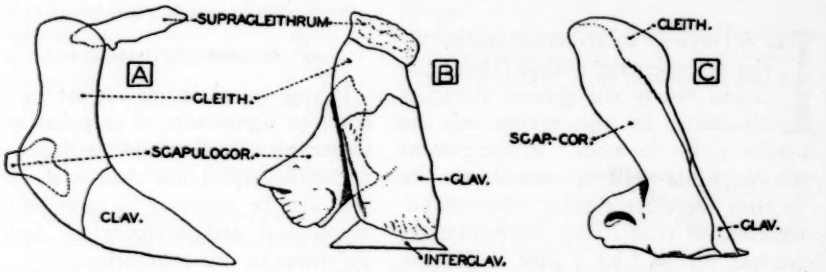

FIG. 1. RIGHT SHOULDER BONES OF (A) EUSTHENOPTERON (BRYANT), (B) EOGERYNUS (WATSON), AND (C) ERYOPS, FROM THE LATERAL ASPECT

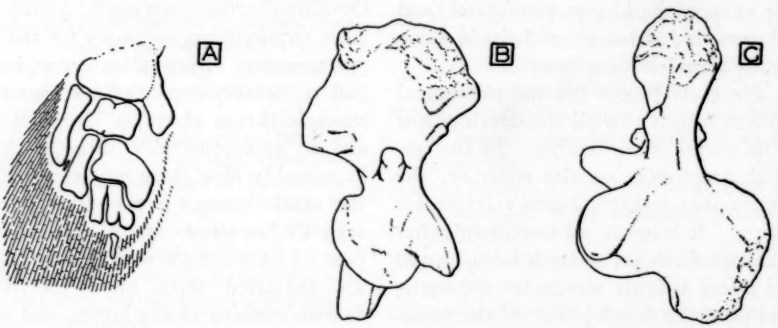

FIG. 2. (A) THE LEFT PECTORAL FIN OF EUSTHENOPTERON (BRYANT), (B) ANTERIOR, AND (C) DORSAL, VIEWS OF LEFT HUMERUS OF ERYOPS (FROM CAST IN AMER. MUS. NAT. HIST.); PROXIMAL END ABOVE

ant (1919) and it is evident that no additional cartilages were hidden by the dermotrichia. On the other hand, *Sauripterus* offers no such difficulty, and it has already been indicated (Howell, 1935) how its fin plan might have been altered to fit the arrangement in *Eryops*.

It is a rule that the pectoral fins of fish have become rotated, with preaxial border of the facts that the radius and the next two elements together are the same length as the ulna, that there is a marginal element upon either fin border that might be considered as prepollex and postminimus, and that in addition there are at least six longitudinal elements, comparable to digits.

Broom (1913) has mentioned the circum-

stance that when fish first took to the land there must have been a lengthy period when the paired fins functioned with fair efficiency as both limbs and fins. In other words the part of the pectoral fin having had chief contact with the ground must have become modified to act as a limb while the remainder of the appendage lagged behind, still being a fin. This would develop an appendage essentially asymmetrical, as was that of *Sauripterus*. This inequality in the length of the ulna and the fact that the latter is so much the more robust in this genus constitutes evidence of considerable weight that this crossopt was partially terrestrial. On the other hand, conditions in *Eusthenopteron*, and particularly the fact that the dermotrichia were still strongly developed over the ulnar border, as shown in Bryant's splendid photographs, may indicate that this genus was not so terrestrially inclined, or possibly that it was retrogressive, having secondarily returned completely to the water.

It will be seen in *Sauripterus* that the carpal bones next distal to the radius and ulna are situated at three different levels (fig. 3), a detail of much significance. If, in *Sauripterus*, one concede the presence of prepollex and postminimus elements shorter than the others, there are left six radialia. For the present it appears advisable to ignore the presence of one of these, assuming either that it disappeared, or that the actual crossopt ancestral to tetrapods had one less radial than *Sauripterus*.

In the transition from fish to amphibian one must consider the probabilities in the post-crossopt stage. Watson (1913) has pointed out that the endoskeletal elements of the appendage proper were originally all of the same order. They could elongate, shorten, or undergo fission or fusion as the stimulus was encountered. It is certainly likely that the radialia of *Sauripterus* later divided into shorter elements, not improbably all of about the same length, as suggested in the diagram of the hypothetical condition in figure 3. It will be noted that the ones next distal to radius and ulna lie at three different levels. It will also be observed that in this dia-

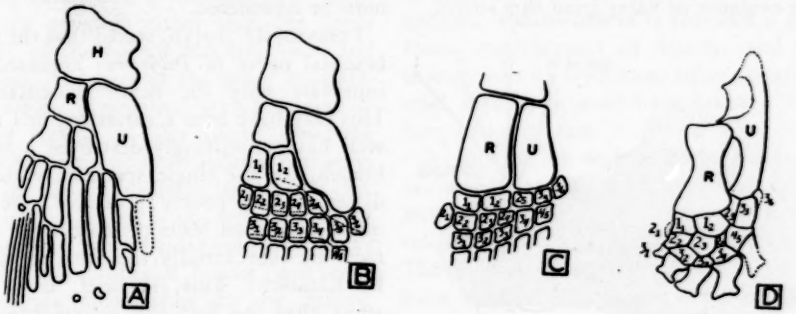

FIG. 3. POSSIBLE EARLY MORPHOLOGY OF THE CARPUS

(A) Lateral view of anterior appendage of *Sauripterus*, transposed to represent left fin (after Gregory): (B) Hypothetical, post-*Sauripterus* stage, showing probable division of carpal cartilages into transverse rows; the broken lines pass through the elements which will be associated transversely in later stages: (C) Hypothetical, pre-*Eryops* stage, indicating arrangement of the carpal elements shown in (C) according to the *Eryops* plan: (D) The forearm and hand of *Eryops* (after Gregory, Miner and Noble), the elements numbered according to their homologues in (B).

gram there has been discarded one of the radialia distal to the carpal element marked 1_1 which appears to be a necessary corollary as above stated. The remainder of the carpal elements are discussed subsequently.

The appendageous skeleton of the living crossopt *Polypterus* is so tangentially specialized that it is hardly worth mentioning in the present connection. This being so, one must look upon the arrangement of its soft parts with considerable suspicion. Yet it is all we have, and if considered on a broad enough basis one may obtain some evidence of value from this source.

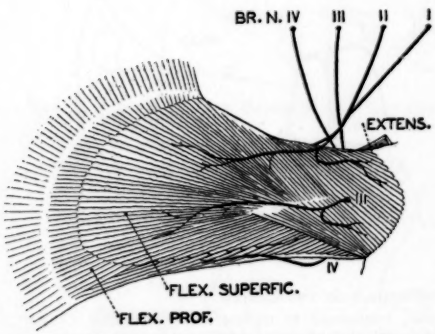

FIG. 4. LATERAL (FLEXOR) ASPECT OF THE RIGHT PECTORAL FIN OF POLYPTERUS, SHOWING TWISTING OF NERVES AND THEIR DISTRIBUTION UPON THIS SURFACE.

In *Polypterus* the pectoral fin musculature is divisible into a superficial and deep extensor muscle medially, and a superficial and a deep flexor laterally, with minor slips of the two divisions. More detailed examination shows, however, that superficially the flexor surface is divided by an irregular raphe into proximal and distal portions (fig. 4) They are functionally separable, although the differentiation of these and other elements is not as exaggerated as indicated in Klaatsch's (1896) figures (extensors and flexors are reversed in his paper). Pulling on the proximal part advances the whole shoulder, and on the distal part, merely the fin proper. Thus the fin musculature may be divided into extensor (medial) and flexor (lateral) divisions, and the flexor mass again divided into shoulder and "brachial" portions. During the "unrotation" of the fish fin to the subsequent amphibian posture this shoulder division of the flexor musculature might exhibit some tendency to remain aligned dorsally with the extensors,—a phenomenon which could explain the later dorsal allocation of the supraspinous matrix. It is not believed that this is so, but it is a possibility that must be considered.

I previously (1933b) stated that the first brachial nerve of *Polypterus* appeared to innervate only the flexor musculature. This may have been a variation, or I may well have unwittingly destroyed a nerve branch, for the single specimen which I dissected was poorly preserved. Subsequent dissection leads me to believe that facts are substantially the same as stated by Klaatsch. This, in brief, is to the effect that the first and second brachial nerves join, and the branches thereof supply both flexors and extensors along the preaxial (more dorsal) border of the fin, including the more preaxial part of the shoulder flexor. The second brachial nerve supplies the central part of the extensor mass and a branch pierces the "coracoid foramen" to the corresponding part of the flexor group, including the more postaxial part of the shoulder flexor. The fourth brachial nerve passes to the postaxial border of the fin and supplies the extreme postaxial part of the flexor complex and slightly more of the extensor surface. These nerves are crossed proximally, in accordance with the way the fin has become rotated. The arrangement is shown in figure 4.

It is believed probable that *Polypterus* offers a fairly precise picture of the basic

arrangement of the brachial nerves, comprising three trunks, all of which go usually to both extensor and flexor musculature, the first to the preaxial fin border, the second to the central zone, and the third to the postaxial border.

One can only compare these nerve elements with those of higher tetrapods in the most general way. Only two ideas can be advanced with any feeling of confidence: the first that if the suprascapular nerve and musculature of higher forms be represented in *Polypterus* it is by the branch of the first brachial trunk going to the more preaxial part of the shoulder flexor, and the other is either that the flexor branch of the fourth brachial nerve and the restricted part of the flexor musculature which it supplies have been lost in higher forms, or (less likely) that the nerve has come to follow another pathway. This conclusion is offered by the fact that in tetrapods there is no nerve to any of the brachial flexors following a pathway around the postaxial border and over the triceps musculature to the flexors. Romer's (1924) conclusion that this flexor branch of *Polypterus* represents the axillary nerve, and hence that the latter and its musculature constitute a flexor component is hardly tenable, for the whole course of phylogeny offers satisfactory evidence that this is a part of the extensor complex. Again, in Romer's illustration of this point (his fig. 10) he compares the course of the nerves of *Polypterus* with higher forms without having first "unrotated" the fin of the former, so that he compares the preaxial border of the fish with the postaxial of tetrapods. An additional point for comment in this figure is that the diazonal nerve of *Polypterus*, and hence its "coracoid" foramen, is not the homologue of the suprascapular nerve and notch in man, nor is either one the homologue of the coracoid nerve and its foramen in modern Caudata, as discussed later.

PROTO-AMPHIBIAN STAGE

It may be assumed that under increasingly arid conditions such crossopts as were best fitted to withstand some desiccation were forced to spend more and more time out of water, travelling from one pool to another. After a long period of time the less adaptable sorts reinvaded the water while the others journeyed for a greater or lesser distance along the line of tetrapod ascent. The step from crossopt to amphibian was not a great one in many respects, but in others it required a profound modification of details, and the change from fin form to an effective terrestrial appendage must have taken a very long period of time.

In fish the fin action involves definite movement at the base, vaguer bending more distally, and spreading of the dermotrichia. Thus there is only one real joint. The journey toward tetrapods seems to have involved an increase of the cartilaginous shoulder girdle at the expense of the membranous elements, and elongation of the more strictly appendageous bones. At some stage between fish and tetrapods the rotation, comprising elevation of the preaxial fin border characteristic of the piscine stage, had to be corrected in order that the volar surface, formerly directed anterolaterally, could be placed flat upon the ground. That the change was in this direction rather than the reverse (placing the dorsum of the manus flat to the ground is indicated by the subsequent arrangement of the brachial nerves.

Before an appendage can be used effectively for terrestrial progression it must develop more numerous joints than occur in fish, and it may be assumed that the ideal for the purpose comprises, in addition to the shoulder joint, a hinge at elbow

and another at wrist. To accomplish the latter the radius and ulna were obliged to have equal length, and the adjoining carpals, which occupy three different levels in *Sauripterus*, would then come to lie in a single transverse row (fig. 3, C). In this row there would then be an external (1_1) and internal (1_2) radiale, an internal (2_5) and external (3_5) ulnare, and a postminimal element (3_6). If one then attempt to reconcile carpal conditions in *Eryops* in terms of *Sauripterus* and of the hypothetical stage B, it will be necessary to assume that all of the elements distal to 1_1 and 1_2 in B became a partial row of medialia, distal to the radius and proximal to the first three digits, and that in addition there was in this row a prepollical element (2_1). The only noteworthy feature of the most distal row of carpals in stage C is that in order to provide one for the fifth digit it appears necessary to assume that this was derived from the same level as the metacarpals of the remaining digits.

The exact manner in which the musculature of the piscine appendage adapted itself to the development of distinct segments and for the control of additional joints is not at all clearly indicated.

AMPHIBIA

Gregory (1915) has considered the cranial similarities among Paleozoic Amphibia to be sufficiently striking to indicate that the transformation from fishes into tetrapods occurred but once. The evidence certainly suggests that the immediate ancestry did not involve diverse groups of fishes, but it is not unlikely that a number of sorts of crossopts, with fin plan varying to some extent, reached the amphibian level.

Amphibia apparently arose in Devonian, if not in Silurian times. From the Carboniferous there are remains of labyrinthodonts or stegocephalians, lepospondyls, and phyllospondyls. The most primitive Amphibia known, according to Noble (1931) and others, are the labyrinthodont Embolomeri. With similar affinities but representing successive evolutionary steps are the Rhachitomi and Stereospondyli, and it is with these that we are at present most concerned. To the Phyllospondyli belong the branchiosaurs and either they or their immediate ancestors are believed have given rise to modern Salientia and Caudata. The Lepospondyli are believed to have given rise to modern Gymnophiona or caecilians.

Although there are certain basic similarities, labyrinthodonts, modern Caudata, and modern Salientia are in many respects three very different groups. In the present state of our knowledge it may be assumed that Rhachitomi were reasonably close to, if not actually on, the line of ascent that led to mammals. Caudata and Salientia, however, have journeyed far in a tangential direction and it is not always easy to tell which of their resemblances are basic and which convergent. There are, however, surprisingly many points which they appear to have in common, though in modified form, with higher vertebrates. When discussing these it must, of course, be constantly borne in mind that all modern amphibians are highly specialized and reflect ancestral conditions only in the broadest respects.

In the present connection many specimens of *Necturus* were studied, certain details of *Triturus*, *Salamandra*, *Cryptobranchus*, *Amblystoma* and several other Caudata were investigated, and a number of *Rana catesbiana* dissected. The later were kindly furnished by the U. S. National Museum.

It is evident that in the most primitive type of tetrapod progression the limbs were not highly mobile but shared with

the body the function of locomotor apparatus. Independent movement of the limbs in the horizontal plane was at first restricted. They were elevated by a rocking movement of the girdle, involving simultaneous protraction of the member, this being accompanied by lateral flexion of the entire trunk. The intrinsic ventral muscles (flexors) then helped to anchor the animal and keep it from slipping backward while the body was bent contralaterally. Undoubtedly the body was first the primary locomotor apparatus on land but the limbs rapidly took over the major part of this function.

The most primitive tetrapod posture is frequently considered to be that in which the appendages sprawled at a right angle to the body axis, with digits directed

FIG. 5. TYPICAL URODELE PROGRESSION (MODIFIED FROM BRAUS)

laterally. It appears that this is fundamentally incorrect. Such a posture is basic for the hind limbs, and for a long time they retain this position in their rôle of supporters for the rear end of the body. The fore limbs became the major locomotor apparatus as soon as they were sufficiently equipped to take over this function from the body. It is probable that at a very early date in their terrestrial phylogeny, if not from the first, they were held in such a posture that the radius was medial to the ulna while the digits were directed forward. This is the position best suited to enable the fore limbs to pull the animal forward. It means that from the very first the stimuli operating upon the musculature were different in the case of the two pairs of extremities, so that the elements of the proximal musculature of the two pairs can not even be analogized, although there is so much basic similarity in the distal musculature. Furthermore, conditions in the shoulder girdle were complicated by the proximity of the head, gills, membranous girdle, and later by the ribs and sternum, none of which is comparable to any influence acting upon the pelvic girdle.

Skeleton

One can be sure that the appendageous equipment of the early Amphibia included a pectoral girdle that had larger endothan exoskeletal elements, a humeral segment, elongated radius and ulna of equal length, flexible elbow and wrist joints, small carpal bones of basically asymmetrical pattern but under a stimulus for becoming symmetrical, and with probably five digits in addition to bordering carpal elements (prepollex and postminimus), the digits of unequal length with a phalangeal formula that should logically have been 2-3-4-5-4 (or 3).

The most primitive amphibian whose shoulder girdle is well known is the embolomerous genus *Eogyrinus* (fig. 1). It had a large clavicle and cleithrum, supracleithrum, and presumably post-temporal and interclavicle, as well as a large "scapulocoracoid." The Rhachitomi, as represented by *Eryops* and *Alegeinosaurus*, had lost the supracleithrum and its articulation with the head. The clavicle and cleithrum were reduced to narrow strips along the anterior border of the girdle, except that the cleithrum was expanded dorsally. The cartilaginous shoulder bone appears to have had but a single ossification, without sutures. Details of interest comprise the facts that the whole girdle sloped somewhat to the rear, the bone had developed forward, upward, and downward from the glenoid region, there was a supraglenoid buttress medially,

probably representing the originally more restricted part of the scapular portion, beneath which there appear to have been two foramina known as supraglenoid and coracoid. Piercing the arch was a small glenoid foramen. Gregory (1915) designates by the term metacoracoid the posterior part of the coracoid. It is entirely permissible, of course, to call this part of the coracoid a metacoracoid process, but it can not be regarded as a separate element, at least until it actually becomes so. It is clear from the fossils that attached to the more ventral part of the complex was an "epicoracoid" cartilage, which perhaps overlapped its antimere at the midline. These details have been discussed by Cope, Miner, Romer and others.

An expanding girdle can encompass any nerve in its path, and similarly a contracting girdle can relinquish nerves. Hence almost nothing can be told about the homology of foramina in diverse sorts of fossils. All that appears positive is that such nerves as pass through foramina of the shoulder girdle are always from the ventral division of the plexus. In such fossil material as *Eryops* it appears unsafe to hazard an opinion whether the nerve piercing any particular foramen was the supra-scapular, coracoid, or one of the nerves to the brachial flexors; or two branches of the same nerve may have passed through as many foramina.

As indicated by the epicoracoid cartilages the two sides of the girdle in *Eryops* had developed some independence of action, and accompanying this was an increase in the mobility of the limbs, which had considerable movement but in a circumscribed pathway, in the horizontal as well as the vertical plane. This is shown by the conformation of the glenoid surface, usually termed screw-shaped, and by the strap-shaped humeral head. Watson (1918) believed that the humerus was incapable of any rotation on its long axis, but Miner (1925) has indicated that rotation to the extent of 90 degrees was possible. At any rate the shape of the glenoid suggests a very rolling gait, with much heaving about of the body.

The humerus of *Eryops* is extremely robust, with bold processes (fig. 2). It has the form of a tetrahedral prism, the proximal expansion being largely attributable to the shape of the articular surface, and the distal, to the great extent of the entepicondyle. The ventral part is greatly expanded, indicating strength of the muscles that advanced and depressed the arm, and similarly the entepicondyle is expanded for great leverage of the antibrachial flexors. Those interested in speculating upon the possible significance and relationship of the various details are referred to Miner's excellent discussions and illustrations. In a well selected series of fossil reptiles may be seen gradations of features that enable the investigator to interpret these in terms of living representatives with considerable accuracy. Such early amphibians as *Eryops*, however, exhibit details that do not occur in living forms, and it appears impossible to reconstruct the muscle attachments with desirable assurance.

The only things distinctive about the forearm bones of *Eryops* are the relatively great development of the olecranon and the distal expansion of the radius. These points, in connection with the great definition of the processes of the humerus, indicate a certain degree of specialization.

The plan upon which the carpus of *Eryops* was organized has already been discussed. Salient features consist of the facts that the bone occupying the position of the first metacarpal seems to have been originally a carpal element, while the carpal subtending the fifth digit was originally a metacarpal. The carpus was thus

essentially asymmetrical, the radial elements occupying a more distal position than the corresponding elements upon the ulnar side of the carpus.

In many respects conditions in modern Caudata constitute something very different from those in the Rhachitomi. Noble (1931) states that in the branchiosaurs there was no ossification of the cartilaginous girdle, but a narrow clavicle and cleithrum, as well as a small interclavicle, were present. It is likely that the basic ossification retained was in the paraglenoid region (Moodie, Gegenbaur, Miner), as a base for the divergence of three cartilaginous processes extending therefrom. But this proves that the branchiosaurs were already far on the road to modern amphibian conditions. It must constantly be borne in mind that the original amphibian condition was one in which both membranous and "cartilaginous" girdles were fused into a solid complex, and it was only later that there was recession in certain spots. Relinquishment of original connection with clavicle and interclavicle by the Caudata allowed those parts giving rise to critical muscles to become still more prominent, or hypertrophied, and permitted still further reduction of nonessential parts. It should be stressed, however, that these were not at first distinct elements but were processes of a single element, developing in accordance with muscle stress. Such processes in different animals can be homologized only providing they are associated with the same muscles, a difficult matter to prove in many fossils.

In modern Caudata there are usually three such processes, all well defined, developed by the elevator (largely), protractor, and depressor muscles of the brachium. The terms scapula, pre- or procoracoid, and coracoid for them have become firmly established without any regard to the musculature involved, but the second is an unfortunate term, for it really is a proscapular process and will be so termed by me hereafter. Suprascapula is the designation employed for the cartilage dorsal to the scapular ossification, and epicoracoid for the cartilage which was situated between the coracoid and midventral line in some of the labyrinthodonts. There is some difference in the application of the above terms, however. Thus Case, Williston and others have used epicoracoid to designate the unossified element occurring anterior to the two coracoids in some reptiles. At best it is frequently difficult to draw a hard and fast line between procoracoid and epicoracoid.

It will be realized that ossification of the urodele girdle as now found is not a case of bone forming from one center and expanding, but the reverse. The modern condition has been brought about by the restriction of the ossification of a once uniformly bony girdle. The bony boundaries have retreated as aquatic specialization dictated, to a greater or lesser extent, and in different situations in diverse sorts. Hence there is much variation in this respect as might be expected, to such a degree that it is difficult to present generalities. Perhaps most frequently there is a single ossification of the scapular constriction, but this may extend to include the paraglenoid region (*Salamandra*), or there may be a separate coracoid bone (*Siren*, Parker; *Pseudobranchus*, Noble). In *Molge* and *Amblystoma* the ossification may even extend to the procoracoid base. All have lost any evidence of a membranous girdle. The reason for this is obscure, although it may be correlated with the reduction in ossification of the cartilaginous girdle because of a weakening of the muscle attachments brought about during an aquatic life.

The coracoid is usually pierced by the

coracoid nerve; hence this is a true coracoid foramen and not the homologue of the diazonal foramen of *Polypterus*, which transmits another nerve. Parker (1868) has stated that in *Salamandra* and *Triton* the nerve passes over the procoracoid-coracoid notch, but in the former genus I found it piercing.

In at least many Caudata there is an anterior expansion of the humeral head continuous with a well defined deltoid process farther distally. The entepicondyle is likewise better defined than the ectepicondyle, and the olecranon also is well developed. On the whole, however, the bones are weak, and the insertion of many of the muscles upon the capsule of the joints rather than upon the bone proper.

The carpus of urodeles is quite specialized, the tendency being toward the fusion or elimination of some of the elements.

Conditions in the Salientia are not of paramount importance to the present study; yet they should be briefly discussed for the sake of completeness. Like the Caudata this group also has, typically, an ossified scapula with cartilaginous suprascapula, although there is frequently calcification of the dorsal part of this (Noble). Upon the anterior border is usually a small cleithrum (Schmalhausen, 1917; Engler, 1929). The rest of the girdle is quite variable but most often there is an ossified coracoid with restricted middle, separated from a slender, bony clavicle and cartilaginous or ossified procoracoid by a fenestration or "obturator foramen." Both clavicle and procoracoid are occasionally absent (Brevicipitidae). At times the three elements are all fused (*Dendrobates*). The procoracoid may be said usually to be bounded by the scapula, coracoid and clavicle, with an acromial process projecting forward. In large, old specimens of *Rana* ossification is extensive and at the medial base of the acromion is a large "supraglenoid" foramen, which transmits minor nerves and blood vessels to the joint. This is located anterior to a prominent ridge, probably representing the "supraglenoid buttress." The procoracoid frequently extends medially, posterior to but attached to the clavicle to join the epicoracoid, or even to fill the space between coracoid and clavicle (*Xenopus*).

The case of the epicoracoid presents an interesting question. The basic, protoamphibian condition was undoubtedly one in which there was solid articulation of the two sides of the girdle. It appears that there was later relinquishment of this anchorage, for independent movement of the two halves of the girdle, by means of a recession of the ossified part of the coracoids from the midline and an overlapping of the cartilaginous parts (epicoracoids). In the Salientia the latter is considered (Noble) to be the primitive condition, not vice versa (Huntington, 1918), while in some more specialized sorts there is a change from this arciferal type to a firmisternal condition, in which the episternal cartilages are fused at the midline. It appears necessary to assume that a similar course was followed by the amphibian, or at least reptilian, ancestors of the Mammalia in order that the coracoid musculature could settle down on the midline of the trunk. When this had occurred there was no longer need or reason for extensive coracoids and they could degenerate to mere processes for the origin of tendons of such brachial muscles as might later be concerned therewith.

There has been considerable argument at one time or another over the question whether the "obturator foramen" of Salientia represents the coracoid foramen or the coracoid-procoracoid notch of Caudata. It seems evident that it represents both. In terms of the tailed amphibians it may be said that the anterior border of

the coracoid receded until the coracoid nerve was included in the notch, and that in Salientia the precoracoid and epicoracoid cartilages then surrounded the space.

In such a form as *Rana catesbiana* the humerus is quite similar to that of Caudata, except for being considerably more robust. Its main crest, divided into a proximal and a more distal prominence, seems largely concerned with the pectoralis, but latissimus, dorsal scapular, and coracohumeral muscles all contribute, while the deltoid has but little to do with it. Upon the medial aspect of the humerus there is a ridge marking the insertion of the coracobrachial muscle divisions.

No modern amphibian has more than four digits in the manus. Schmalhausen (1917) demonstrated additional elements in the larval *Bombina*, but it is doubtful if these are more than carpal elements.

Nerves

As previously discussed, and as will be amplified later, the nerves are considered always to offer reliable information regarding the relationship of muscles. The information which they furnish must be used with intelligence, however, for conditions are often obscured. Nerves are subject to certain minor, or even major (facial and trigeminal fields) changes, which would be clear if one could examine the complete phylogenetic record. One should be careful not to make such mistakes as to assume that the first nerve to split off from the plexus anteriorly must be the same in all cases, as has frequently been done.

As compared with fish the Amphibia exhibit a bare beginning of the posterior migration of the brachial plexus that is farther advanced in reptiles and mammals. Basically the plexus may be said to be formed by three cords, as in *Polypterus*, these coming in Caudata from spinal nerves III, IV, and V. In the latter the middle one or fourth is usually more robust than the other two combined. The second nerve may also contribute but it is probable that this usually carries only hypobranchial fibers. At least when such an anastomosis is present in *Necturus* there appears always to be a hypobranchial branch given off from the combined nn. II and III as well as one from n. II alone. Rarely there may be a similar contribution to the plexus from VI (*Megalobatrachus*, Fürbringer, 1879), the derivation then being from five nerves, simulating the condition in mammals. But still the two upper nerves fuse, as do the two lower, forming three cords.

Fundamentally, then, it is believed that the tetrapod plexus is derived from three cords, coming from a number of nerves varying from three to five. The major alignment is next into dorsal and ventral divisions, supplying respectively the original extensor and flexor musculature. In all tetrapods these are relatively easy of determination. Fundamentally it should probably be considered that the dorsal division of the appendageous musculature, and hence the nerves, is separable into an extrinsic, suprazonal (or supracleithral) group (serrati), another group concerned with the shoulder, a third brachio-antibrachial group for moving the forearm, and of course elements for podial action. From each of these, particularly the second, certain subgroups later take form, their nerves exhibiting a greater or lesser degree of distinctiveness. In this category fall the nerves to the latissimus, dorsalis scapulae, and proscapulohumeral divisions.

Concerning the nerves of such subgroups of muscles there is often a definite trend, which might well be dignified by the term law. This is to the effect that if some particular division of one subgroup be so

situated as regularly to act with another subgroup, its nerve will exhibit a tendency to align itself with the innervation of that subgroup, if no intervening obstacle impose. Hence the innervation, particularly in one of the higher vertebrates, is not always precisely diagnostic of the origin of a muscle, or rather the facts may be thus obscured. In line with this reasoning is the apparent fact that high specialization restricting the diversity of arm movements will tend to result in a greater homogeneity of the entire plexus (Salientia).

Basically the nerves to the extrinsic muscles of the limb are given off from cords before the plexus proper is reached, as noted by Detwiler (1920). Thus there are fine twigs from the nerve roots supplying the slips of the serrati, including levator scapulae, and subsequent branches to the latissimus and pectoralis, while the trapezius and omohyoid have other innervation.

A discussion of the brachial nerves of the Caudata, as typified by *Necturus*, may be presented as follows:

N. vago-accessorius, so-called, is a small branch diverging from the vagus and innervating the m. trapezius.

Nn. hypobranchiales, from either sp. n. II alone, or when there is anastomosis of this nerve with III, then a second branch from below their junction, to the hypobranchial musculature, including the three slips connected with the girdle.

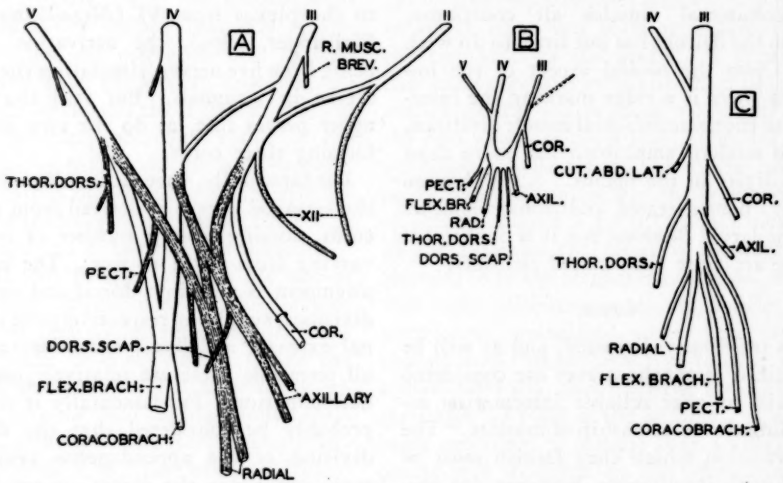

FIG. 6. (A) LATERAL VIEW OF COMPOSITE, RIGHT BRACHIAL PLEXUS OF NECTURUS, SOMEWHAT IDEALIZED; PROXIMAL PART SHORTENED, DISTAL PART ELONGATED; DORSAL ELEMENTS SHADED, HYPOBRANCHIAL (XII) ELEMENTS STIPPLED. (B) AN EXTREMELY COMPACT TYPE OF BRACHIAL PLEXUS OF NECTURUS. (C) LATERAL VIEW OF RIGHT BRACHIAL PLEXUS OF RANA; PROXIMAL PART SHORTENED

Dorsal (extensor) division

Suprazonal group

Rami musculares breves, from all brachial roots, supplying slips of the serratus and levator scapulae.

Shoulder group

N. thoracodorsalis (latissimus dorsi) usually diverges from n. V before the latter reaches the plexus, but at times it comes from the plexus direct. It supplies the

mm. latissimus dorsi and usually the dorsitriceps. With it is sometimes associated a small nerve to the posterior part of the m. dorsalis scapulae.

N. axillaris communis supplies the rest of the dorsal muscles whose chief bulk lies proximal to the shoulder joint. It might with equal propriety be considered as the upper division of the radial. In some urodele plexuses it is totally distinct from the latter, and in such case is derived from nn. III and IV. It supplies the mm. dorsalis scapulae, scapulohumeralis brevis, and proscapulohumerales longus and brevis. At times twigs to some of these muscles, particularly the first, diverge above the point where the main axillary separates. In some cases it also conducts twigs to some of the triceps divisions.

Brachio-antibrachial group

N. radialis (brachialis longus superior) is derived from nn. IV and V, and possibly at times from III as well. It follows the usual course, piercing the triceps, but is variable in its branches. Not infrequently there is a preaxial branch extending down the radial side of the antibranchium, and a postaxial branch down the ulnar side, the latter unrepresented in mammals as a definite entity. This, where it occurs, appears to be more concerned with the brachial extensors and the other with the antibrachial. Occasionally the two branches have separate origin from the plexus. The triceps may be said to be so innervated, usually with the exception of the dorsitriceps element. A second branch to the dorsalis scapulae often diverges from the n. V contribution to the radial. This appears to occur only where the other nerve to this muscle comes from IV alone and has no especial significance.

Ventral (flexor) division

Shoulder group

Here belongs a discussion of the n. suprascapularis of mammals, which is unrepresented as such in modern Amphibia, for there is no brachial nerve passing anterior to the scapula. Authors have frequently implied, if not actually stated, an opinion that the anterior part of the innervation of the m. dorsalis scapulae has in mammals changed its course and position from posterior to the scapula to one carrying it anterior thereto. This is not sound morphology. If such a change took place it would have to be gradual, involving migration of a foramen transmitting the nerve, from one border of the scapula to the other, and there is no slightest evidence that this ever occurred. Nerve pathways are conservative and when a muscle shifts sideways it drags its nerve along, contorting the old pathway, perhaps, but not taking short cuts. Hence this possibility must be ruled out.

It has never been determined whether the suprascapular nerve of mammals belongs with the dorsal or ventral division of the plexus. In man Kerr (1918) found that it may come from either. Some anatomical text books incline to one view and others to the opposite. I have had the question constantly in mind during dissections but have been able to arrive at no conclusion. There is this to be considered, however; the nerve is variable in this respect, hence it is permissible to entertain the theory that it is in course of change. Its musculature is aligned with the dorsal muscles; hence if it were originally a dorsal group the nerve would have no occasion to alter its relationship. If it were originally a ventral muscle group its nerve might very well exhibit this tendency for allocation with the adopted dorsal division. Hence this evidence suggests, but does not prove, a ventral derivation.

With the above in mind there appear to be only three possibilities at all tenable for consideration: a) that the suprascapular element, originally present in early ancestral tetrapods, has been lost by modern Amphibia, and Reptilia (apparently); b)

that it was derived from the axillary matrix; and c) that it was derived from the coracoid complex.

a) It is tempting to consider that during the "unrotation" of the crossopt fin the more dorsal and proximal part of the flexor (lateral) musculature was left in a position more dorsal than the remainder of the flexors, and thus excellently situated for ultimately becoming the suprascapular group. The fact that the group is absent in living reptiles and amphibians is no disproof of the theory for they may represent lines that early lost it. If this were so, however, the nerve would have passed between membranous girdle and scapula, leaving in fossils of early tetrapods an unmistakable scapula foramen. There is no record of any such foramen and the theory therefore appears unsound.

b) Derivation from the axillary matrix would mean not only that the suprascapular musculature belonged originally to the dorsal division, but that it has migrated anteriorly from the axillary region along the medial surface of the scapula and then over its dorsal border to the present mammalian position, as indicated by present innervation. This is not impossible, morphologically speaking, for a similar situation is found in the facial field, and its nerve would be expected eventually to diverge from the plexus higher up so that it might well simulate the position of the suprascapular nerve. But it is unlikely that such a roundabout course of migration would be adopted when a more direct one was available (but was not followed).

c) The essential features of suprascapular muscle derivation from the coracoid matrix have been advanced by Romer (1922), and at present this appears to be the most attractive theory for tentative adoption. At first glance it may not seem likely, but further consideration, in connection with the situation in reptiles especially, is more convincing. It would involve dorsal migration of the origin of the most dorsocranial division of the coracoid musculature, passing medial to the clavicle and acromion so as to invade the anterior base of the scapula; hence beneath a part of the deltoid. Its nerve would then logically tend to diverge from the plexus above the original n. coracoideus, and after long association with dorsal elements, might well exhibit a definite trend toward allocation with that division, as it now seems to do. Further discussion of the problem will be deferred for the contribution of this series concerning reptiles.

N. pectoralis appears to have the same relationship to the n. coracoideus as n. thoracodorsalis has to the axillary complex. It is always very distinct and arises from n. V, usually above the plexus but at times as an upper branch from the common flexor nerve. It supplies the true pectoral musculature.

N. coracoideus (supracoracoideus) arises from n. III, usually above the plexus, and is a very distinct entity. It pierces the coracoid cartilage by one or more foramina (said by Parker (1868) to pass anterior to the coracoid in *Salamandra* and *Triton*, but I found it piercing in the former genus) and supplies the two divisions of the coracohumeral musculature.

Brachio-antibrachial group

N. coracobrachialis. This nerve may be considered as consecutive with the n. coracoideus but it could not recently have split off that element, else it would pass through the coracoid foramen. It arises probably from n. IV and perhaps at times from III as well, separates above the axillary region and supplies mm. coracobrachialis and gleno-antibrachialis.

N. flexor brachii (brachialis longus inferior) arises from nn. IV and V. It

passes in the brachium deep to the m. gleno-antibrachialis, and after giving off a twig to the m. brachialis, divides into a component corresponding to r. cutaneus antibrachii lateralis n. musculocutaneus of mammals, and into median and ulnar branches which pass to the forearm.

In the Salientia the brachial plexus is secondarily much simplified, but complicated for the student. There is a tendency for the restriction of the brachial area of the spinal cord which is in line with similar restrictions posteriorward, at least one of the contributing causes for the condition probably being the fact that anurans do not habitually indulge in a great variety of movements.

So far as I know no member of the Salientia has more than three nerves contributing to its brachial plexus, while that from the anteriormost may either carry hypobranchial fibers, be very small indeed, or not join at all. The second (n. III) is enormous, and the third (n. IV) very small. Hence, not only is the brachial center one segment farther forward in Salientia than Caudata, but it is more concentrated in the former. Junction of the two (or three) nerves occurs very high and there is then a long, stout cord. Even after many of the branches separate they are contained for a space in a common sheath.

A discussion of the brachial nerves of the Salientia, as typified by *Rana catesbiana*, may be presented as follows:

With discrimination dorsal and ventral nerves, but disposed more in an anterior (flexor) and posterior (extensor) manner, may be distinguished.

N. vago-accessory, so-called, is a branch of the vagus innervating both mm. trapezius and interscapularis.

N. hypobranchialis, from sp. n. II, sends a branch to the muscle termed provisionally omohyoid.

N. cutaneous abdominis lateralis is mentioned because of its prominence. It is evidently derived from n. IV alone and is given off the main brachial trunk posteriorward to the side of the body.

Dorsal (extensor) division

Suprazonal group

Rami musculares breves, from II, III and IV, innervate the eight slips of the serratus matrix, including the two rhomboid and two levator scapulae elements.

Shoulder group

N. thoracodorsalis, like all subsequent nerves, is given off the main trunk and is the highest of the extensors. It innervates the latissimus but usually has also a branch near its base, the ramus posterior dorsalis scapulae, to that muscle. The association of this ramus with the base of the nerve may have but little significance.

N. axillaris usually comes off the cord next. It supplies the superficial and deep parts of the deltoid, and usually the anterior innervation of the dorsalis scapulae, although the latter branch not infrequently leaves the trunk independently and just above the axillary.

Brachio-antibrachial group

N. radialis may be said to have two divisions, one mostly to the brachial and the other to the antibrachial flexors. The former may occur as several separate twigs.

Ventral (flexor) division

Shoulder group

As in the Caudata the suprascapular nerve as such is absent in the Salientia.

N. pectoralis comes off at the base of the brachial flexor nerve, with which it is doubtless secondarily associated because the action of its muscles is apparently so intimately related to that concerned with brachial flexion. It accompanies the bra-

chial flexor nerve beneath the origin of the caput scapulare tricipitis before it innervates the three (really four) divisions of the true pectoral musculature.

N. coracoïdeus is the first ventral nerve given off the brachial trunk. It passes through the "obturator" foramen and supplies the coracoradial and the two parts of the coracohumeral musculature.

Brachio-antibrachial group

N. coracobrachialis appears to lack some of the elements of this nerve in Caudata. It frequently diverges from the brachial trunk above the n. pectoralis and supplies the three divisions of the mm. coracobrachiales.

N. flexor brachii diverges from the n. radialis before the capsule of the shoulder is reached and passes just deep to the origin of the caput scapulare m. tricipitis. At the anterior border of this muscle it becomes entirely superficial and so continues down the front of the brachium until it disappears between the extensor and flexor groups of the antibrachium, but not having a branch characteristic of a n. ulnaris. Its superficial course in the upper arm is attributable to the fact that the coracobrachial musculature is confined to the proximal end of the brachium.

Muscles

The muscles of the appendages are arranged in definite groups. Topography is, of course, often of considerable value in the identification of some individual units, but it is often extremely misleading. Not only may the situations be different, but also muscle attachments can alter greatly by initial retreat, by lateral shift, and finally by secondary advance, or in other ways, so that the only trustworthy criterion is the innervation. Even the latter may tend to lead one astray because of the fact that some units of one group may act with a second, and other units with a third group of muscles, the respective nerves then tending to align themselves with those of the adopted units. Conditions must then be interpreted in terms of phylogeny.

As already indicated in the discussion of nerves, different groups of muscles have different values and any exact arrangement must include many points that are subject to arbitrary decision. Thus it is a matter of individual opinion whether one considers that the dorsalis scapulae and procoracohumeralis belong to one group or two.

The conformation of the musculature during vertebrate phylogeny indicates a trend toward certain, definite patterns characteristic of particular stages. That found in modern Amphibia may not represent an ideal, but is probably about as close to it as the limitations of the organism have permitted. The extrinsic system for operation of the girdle appears to have comprised a protractor (levator scapulae) and retractor of the dorsal part, both derived from serratus slips. The retractor portion later changed to elevator (rhomboid) and depressor (true serratus). Elevation and protraction of the central part of the girdle were effected by the trapezius, and protraction more ventrally by hypobranchial slips. These, of course, attached originally to membranous components of the girdle. Retraction with elevation of the glenoid region was effected by the latissimus, and retraction with depression by the pectoral musculature, both probably inserting fundamentally upon the capsule of the joint rather than any particular bone. There are thus basically five extrinsic groups of muscles operating upon the shoulder girdle, the posterior two being dual-purpose groups, acting upon both girdle and brachium.

Modern urodeles have certainly had

sufficient time to adapt their musculature thoroughly to an aquatic habitat, and they illustrate an arrangement for control of the brachium that may be considered as fairly ideal for their type of life. This comprises five groups of muscles for moving the arm in as many directions, as already pointed out by Rylkoff (1924), consisting of scapular musculature, with action just cranial of dorsal; procoracohumeral group, cranialward; coracoid group, just cranial of ventral; pectoralis, caudoventralward; and latissimus, caudodorsalward. It is probable that the original musculature of the first three of these groups had origin also from the membranous components of the girdle, while the last two are, or at least early became, extrinsic muscles probably acting largely on the capsule of the joint as already mentioned. Several of these groups either had, or easily developed, shorter slips to effect a greater variety of brachial movements.

It is felt that in modern amphibians the brachium is situated in a somewhat anomalous position, for it furnishes a common attachment for the longer muscles of the shoulder and the muscles passing to the forearm. A higher plan of extensor (triceps) control has begun to be well organized, but the brachial flexors exhibit poor organization. Perhaps the movements needed have been too simple to furnish an adequate stimulus, or the original equipment provided was difficult to alter.

There is a great deal of well illustrated literature giving in greater or lesser detail particulars regarding the anterior limb musculature of modern Amphibia, so that the topography is thoroughly known. Some of these contributions carefully describe the peripheral nerves as well. The older of them are usually quite worthless from a morphological viewpoint because so little was known regarding nerve-muscle relationship at the time they were written. Even such excellent reports as that of Wilder (1912) are of no help in morphological determinations because the nerves are ignored.

A second type of myological publication is that wherein the musculature of fossils is reconstructed. An outstanding instance is the report of Miner (1925), a truly masterful presentation of his subject. His discussions are invaluable, and yet it is the individual opinion of the present author that but little reliance can be placed upon any restoration of muscles of fossils, no matter how expertly done, because of the impossibility of accuracy in the more detailed points, which are usually the most important ones. In fine, it is considered that but little can be told of the more significant details of the muscular conditions in *Eryops*, and that most of this is uncertain. Only broad generalities can be presented with any confidence.

As already stated some of the muscles of Caudata and Salientia can with certainty be homologized with those of mammals. Others can be so treated with considerable probability, while in a few cases there is more uncertainty. They both arise from the same matrix, however, and although very possibly they might not both have come from precisely the same part of the matrix, they may properly be called equal, *providing* any existing uncertainty is clearly stated. The muscular conditions of *Necturus* can be compared with those in mammals with more assurance than *Necturus* can be compared with *Rana*. In view of the high specialization of the Caudata it is really astonishing that one can homologize the more significant details of their musculature with those of mammals so satisfactorily.

Branchiomeric division

M. trapezius (cucullaris) very clearly represents the posteriormost element of

the original gill arch musculature. Because of the facts that branchiomeric slips to the membranous girdle in fish are variable and that other slips with myomeric derivation in this group have at times been termed trapezius in spite of somatic innervation, this detail reaches the amphibian stage with more uncertainty attached to it than the facts warrant. In modern Amphibia it appears always to be uncomplicated by division and a sternocleidomastoid element is consistently lacking. The trapezius arises either from the superficial fascia between head and girdle (numerous Caudata), or has a tendency to shift to the head (Salientia). In *Necturus* it is quite broad, and in *Rana* narrow but thick. In the former it inserts at the anterior base of the scapula and in the latter upon the dorsal base of the acromion—especially the same situation. There can be little doubt but that the insertion of this muscle was basically upon the cleithrum, perhaps quite broadly so, as believed by Romer and Miner, and this may well have included a part of the clavicle. As it is now found in amphibians it has evidently shifted to the border of the scapula and relinquished the more dorsal part of its attachment, as well as any connection with the clavicle, as the cleithrum became severely restricted or disappeared. In its rôle of elevator and protractor of the shoulder it is one of the most important and constantly used appendicular muscles of tetrapods in general.

Miner described for *Megalobatrachus* a broad levator scapulae innervated from both spinal and vago-accessory sources. It is clear that the dorsal part of this sheet was narrow with attachment well away from the scapula border, and constituted a perfectly normal levator scapulae. Extending ventrally from this was a relatively broad sheet with origin from the pharyngeal wall and aponeurotic insertion upon the scapula border. It seems that this was the part with visceral innervation. The conclusion which this circumstance led him to adopt was that the history of the levator scapulae is similar to that of the trapezius. The suggestion was offered that this represents the deeper portion of branchiomeric slips found in Holocephali and Selachi; that the trapezius and levator scapula represent in urodeles the remainder of the branchiomeric system and as in the case of the trapezius there has been a taking over of the levator scapulae by the somatic system, as yet uncompleted in *Megalobatrachus*, the stage in the latter being comparable to the stage of the trapezius in higher forms.

The condition of this sheet as reported by Miner is unique among vertebrates. Not only has a broad levator scapulae partially innervated by a visceral nerve never been described in other urodeles, but it was not found in *Megalobatrachus* by Humphry (1872) and Osawa (1902), and it is not the case in the *Cryptobranchus* which I have dissected. Certainly there are insufficient grounds for considering that the levator scapulae was originally a derivative of the gill arch musculature, or that Miner's levator scapulae is not a compound element. The logical conclusion is that a vagrant visceral slip, comparable with the interscapularis of anurans, has secondarily become associated, where such a condition exists, with the myomeric levator scapulae.

M. interscapularis is found only in the Salientia. It is fan-shaped, arising from the more anterior part of the medial surface of the suprascapular cartilage and converging, in the region of the supraglenoid buttress, to an insertion upon the base of the scapula, caudally adjoining the trapezius. Its function is to bend the dorsal part of the girdle in a medial direction, but the importance and degree of such

movement appears to one as insufficient to account for its strength. Its position suggests that it may have been derived from the settling down upon the suprascapula of a part of the trapezius; but it is better to consider it merely as a division of the

Hypobranchial division

This is quite variable, and it is felt that conditions in amphibians should not be compared to those in mammals with too much exactitude.

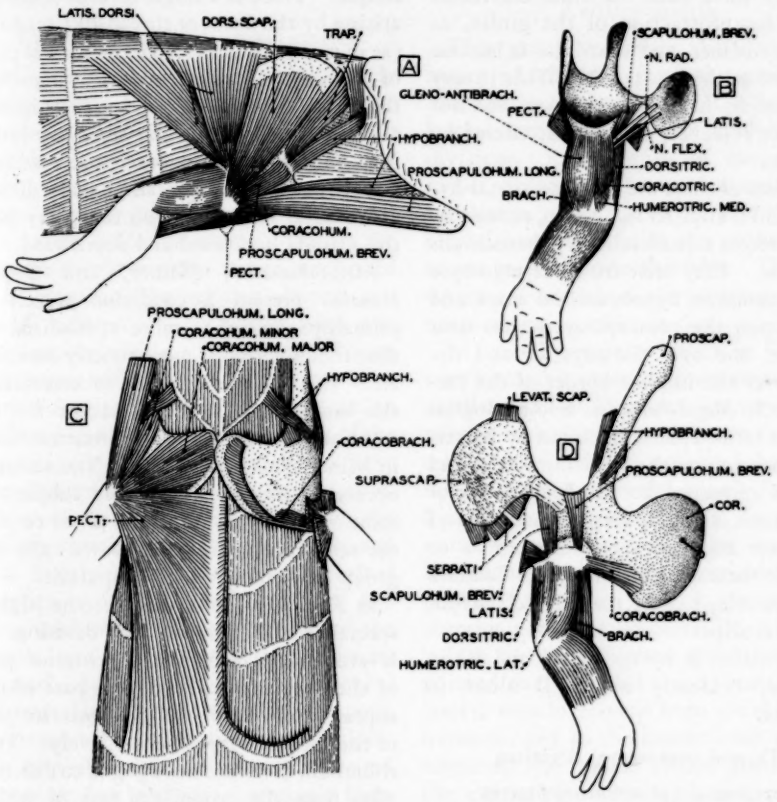

FIG. 7. THE SHOULDER REGION OF NECTURUS

(A) Right lateral view; (B) Medial details of right brachium and of shoulder girdle; (C) Ventral view; (D) lateral details of right brachium and deeper musculature of the lateral girdle.

branchiomeric system, without more precise commitment.

Innervation is by a separate twig of the vago-accessory branch supplying the trapezius.

M. omohyoideus may or may not be equal to the muscle of the same name in mammals. In *Necturus* it arises from the interbranchial fascia and inserts upon the border of the scapuloproscapular notch. In

Megalobatrachus it is broader and joins the sternohyoid (Miner). In *Rana* it arises from the hyoid and passes medial to the girdle and the m. interscapularis to an insertion upon the posterior border of the scapular base. It appears that originally this may have been the chief clavicular muscle for protraction of the girdle, as stated by Miner, and that later it became identified with the scapula. This theory is reached by inference, however, and not by direct evidence, except that offered by *Sphenodon*.

In *Necturus* there are two additional hypobranchial slips to the girdle, termed by Wilder rectus superficialis hypobranchialis posterior. They arise from the myosepta of the common hypobranchial mass and insert upon the proscapular process near its base, one over the superior and the other over the inferior border of the cartilage. In *Megalobatrachus* two analogous slips are termed sternohyoideus by Miner. They insert upon the scapular border and sternum respectively. In *Rana* posterior attachment is to the sternal cartilage. I prefer not to offer a commitment as to whether these are homologous in Caudata and Salientia, calling them merely hypobranchial slips.

Innervation is by sp. nn. II and III in Caudata; evidently by n. II alone in Salientia.

Dorsal (extensor) division

Suprazonal (or serratus) matrix

This may be divided into levator scapulae, rhomboid, and true serratus slips, and there appears to be no doubt but that all arose from a common group of several slips arising from the myosepta and most probably inserting originally on the dorsal part of the cleithrum, or even supracleithrum, secondarily becoming associated with the suprascapula, and the scapula also in Salientia. The original inclination it is impossible to tell, but the anteriormost, becoming the levator scapulae, probably extended toward the head at a very early date, while the others became disposed at various angles.

In *Necturus* the arrangement is relatively simple. There is a single levator scapulae arising by the finest of thread-like tendons (as in many Caudata) from the medial part of the occipital region, and inserting upon the cranial angle of the suprascapular cartilage. Rhomboid divisions are lacking. There are three serrati proper arising from as many myosepta and having dorsal and cranial inclination, so that they pull the scapula backward and downward.

Megalobatrachus (Miner) and *Cryptobranchus* present a condition which is probably somewhat more specialized in that the direction is more strictly fore and aft. The levator scapulae is essentially the same as in *Necturus*, except for its association with a branchiometric unit in Miner's *Megalobatrachus*. The serratus occurs in five slips (number subject to some variation) so disposed as all to pull the scapular backward. Osawa calls the group as a whole thoraciscapularis.

In *Rana* this group has become highly specialized. There are two divisions of levator scapulae from the posterior part of the cranium to the medial part of the suprascapular angle and the posterior part of the inferior scapula respectively. Two rhomboid divisions have begun to differentiate from the group, the first of which is associated with the levator scapulae superior, and the second, or posterior, with origin from the midline, inserts by a common raphe with the most dorsal of the serrati. The latter are four in number, three with the usual attachments and the fourth inserting far down upon the posterior base of the scapula. It is relatively powerful. It may well be that the chief use of this muscle group in the frogs is

for easing the shock of landing after a long hop.

Innervation of the levator scapulae is by sp. n. II (at times possibly by n. III as well) in Salientia; the remainder of the complex by nn. III, IV and V in *Necturus*, and by nn. III and IV in *Rana*.

Thoracodorsal (latissimus) matrix

M. latissimus dorsi is always clearly recognizable as such throughout the tetrapods, hence for convenience it is considered as constituting a separate group, although its relationship to the dorsal scapular muscle is so close that this is a purely arbitrary course. Unlike the other intrinsic muscles so far mentioned it is uncertain whether this element primarily was at all concerned with the membranous girdle. Of less importance is argument whether it had extrinsic or intrinsic origin. This, after all, is an academic question, for every muscle of the appendage is believed originally to have been derived from the trunk musculature. At least it must have diverged from the axillary matrix at a very early date, and it is highly probable that the original insertion was upon the capsule of the joint, with slight encroachment upon the scapula or (and) humerus as conditions dictated. Its early definition was doubtless stimulated by the need for a muscle to retract, while elevating, the shoulder.

In amphibia this muscle may be broad or narrow, but it always has origin from the dorsal fascia. Insertion in Caudata should probably be considered as partly upon the capsule of the joint, and partly upon a raphe common to this muscle and the dorsitriceps, although conditions may vary to some extent within the order. In *Rana* insertion is by a stout tendon shared with the dorsal scapular muscle and inserting upon the pectoral ridge of the humerus.

Innervation of the latissimus and usually dorsitriceps in Caudata is by a separate branch from n. V, but at times the latter muscle is supplied by way of the radial. The latissimus in Salientia has similar innervation.

Axillary matrix

This group comprises the dorsal elements which arise from the girdle and insert upon the humerus. It includes the lateral scapular and proscapulohumeral divisions. Relationship of the first is rather intimately concerned with the latissimus, while one can be sure that the second originated from the common extensor matrix of the shoulder. The rank which one accords to the separate components is thus an arbitrary decision.

M. dorsalis scapulae arises in Caudata over a variable extent of the suprascapular cartilage and in *Necturus* it converges to a narrow insertion upon the anterior (ventral) process of the proximal humerus. In *Rana* it is a powerful muscle from the basal half of the suprascapula, developing a strong tendon which joins with the latissimus tendon to insert upon the pectoral ridge of the humerus. The anterior part may be partially separable.

It is likely, as Watson and Miner have pointed out, that this was at one time *the* lateral muscle passing from cleithrum to humerus, and as the former bone became restricted the muscle settled down upon the expanding suprascapula and scapula. It is quite closely related to the latissimus, and particularly to the proscapulohumeral subgroup. Throughout the literature it is rather consistently considered as a part of the deltoid, but it does not enter into the composition of the deltoid of mammals. It is also evident that it has nothing to do with the true suprascapular musculature of mammals. As the latter group is lacking in modern Amphibia the

dorsalis scapulae has spread over the whole extent of the dorsal girdle component. At a later tetrapod stage the suprascapular musculature increased at the expense of the dorsalis scapulae, crowding the latter to the posterior border of the girdle, where how the two muscles have been separated by the long head of the triceps, and it becomes apparent that the dorsalis scapulae is the forerunner of the teres minor. The innervation also bears this out. Furthermore, this is the only muscle of

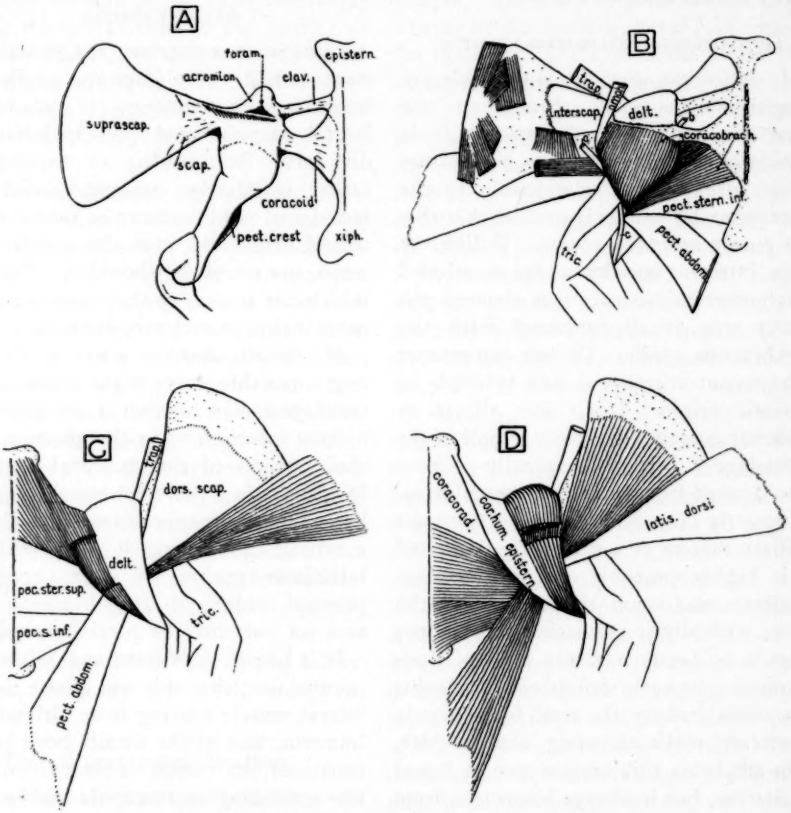

FIG. 8. DETAILS OF THE LEFT SHOULDER OF RANA CATESBIANA

(A) Medial view of skeletal elements; (B) Medial view of musculature; (C) and (D) Lateral view of musculature. Muscles depicted, shaded or unshaded by phylogenetic groups.

it becomes in mammals one of the teres divisions. In *Rana* the dorsalis scapulae appears to have all the qualifications of a teres major, inserting, as it does, with the latissimus. In *Necturus* one is not so sure, while in reptiles and mammals one can see cold blooded tetrapods that could become the teres minor without a great amount of alteration of its insertion.

M. scapulohumeralis brevis. This apparently is not the scapulohumeralis muscle of Miner, but his subcoracoscapularis, a

term which I regard as misleading because in amphibians it has nothing to do with the subcoracoid area. In *Necturus* it is located laterally adjacent to the brachial nerves, with origin from the caudal border of the scapular base, and rather lengthy insertion upon the shaft of the humerus. In *Rana* there is a very short, small muscle evidently representing this element arising by a tendon from the posterior border of the scapular base and inserting fleshily upon the adjoining part of the neck of the humerus. It is hidden by the deltoideus profundus and dorsalis scapulae, and is not illustrated. This slip and the scapulohumeralis brevis offer certain difficulties which will be discussed under the heading of the latter muscle. Suffice it here to say that the former element appears to contribute to the subscapularis of mammals.

M. proscapulohumeralis longus (procoracohumeralis, deltoid Romer, deltoideus clavicularis Miner). This division is at times termed procoracohumeralis, but as it is a dorsal muscle this is a misnomer. In *Necturus* it arises from the lateral surface of the distal half of the proscapular process except near the tip, and inserts upon the anterior (ventral) process of the proximal humerus. It is perfectly distinct from the coracoid group, but in other forms, such as *Salamandra*, the two give the superficial appearance of being a single sheet.

In *Rana* I have termed this division deltoideus superficialis. It is much specialized, as it has invaded the medial aspect of the girdle, with origin really from the medial base of the coracoid. Thence it passes over the acromion, to which there is extensive attachment, and insertion is upon the medial epicondyle of the humerus, a further specialization.

There is essential agreement among authors that this represents a part of the deltoid, and to my mind it is the sole representative. Basically with origin from the clavicle it has settled down upon the procoracoid as the clavicle disappeared in the Caudata, and has stimulated that cartilaginous process to great extension in some forms. In *Rana* it has left the clavicle, evidently for a more favorable angle of action from the acromion. It has undoubtedly been instrumental in the definition of this process, but has secured greater length by encroachment upon the medial side of the girdle than the acromion has perhaps been capable of effecting, and by extension down the arm.

M. proscapulohumeralis brevis (procoracohumeralis, scapulohumeralis anterior). Miner found this deep division well developed in *Megalobatrachus*, and it is clearly recognizable, although rather weak, in *Necturus*.

In *Rana* I have termed deltoideus profundus an incompletely separable division of the deltoid which is comparable in character with the proscapulohumeralis brevis of *Necturus*. It is entirely deep to the superficial deltoid, arising from the acromion and border of the scapuloacromial notch. It passes beneath (medial to) the latissimus tendon and inserts upon the posterior base of the pectoral ridge of the humerus. In some Salientia the border of the proscapulohumeralis presents the appearance of having fused with the adjoining coracoid division (Anthony and Vallois, 1914), and a similar condition may be encountered in Caudata (*Salamandra*). Neither Miner nor I attach any special significance to this fact. Superficial fusion of two muscles constantly occurs in phylogeny and seldom proves to have more than transitory significance.

Romer considers that this slip represents a teres minor, but a consideration of the insertional relations of the several muscles

of the shoulder indicates very clearly that the amphibian representative of that muscle is the dorsalis scapulae. What then does it represent? It was considered at first that it is the homologue of the teres major, as its insertion conforms to this thesis and the origin could easily migrate. A major difficulty in the way of this hypothesis, however, lies in the fact that it is separated from the latissimus dorsi by the axillary nerve, which is not the case with the teres major of mammals. It is possible, of course, that the nerve migrated through the muscle, but there is no evidence that this occurred. Hence it is wiser to consider that the teres major is derived from the latissimus, as Romer believes. Consequently it appears necessary to believe either that the proscapulohumeralis brevis and scapulohumeralis brevis have both contributed to the m. subscapularis, or that one of them—most probably the former—has disappeared in mammals and is unrepresented.

This entire group of muscles, comprising all dorsal divisions of the intrinsic system of the girdle, is innervated by branches considered to comprise a homogeneous, axillary group of nerves, whether they occur as one main branch or some of them diverge from higher up, as they occasionally do. A point to be considered is that the dorsalis scapulae is at times supplied by two twigs. This may indicate a tendency for the muscle to divide into two parts, but may have no more significance than have the separate branches of the radial innervating the triceps.

Brachio-antibrachial matrix

In *Necturus* there are four divisions of the anconeus, or as I prefer to call the group, triceps, all with insertion upon the elbow, as follows:

M. humerotriceps lateralis (anconaeus humeralis lateralis) is the lateral division adjoining the flexor group, with origin from the head of the humerus.

M. dorsitriceps (anconaeus scapularis) is the division next posterior, with origin from the tendon of the latissimus; but there are a few muscle fibers extending from the glenoid margin to the latissimus tendon, which may represent a more proximal part of the division.

M. humerotriceps medialis (anconaeus humeralis medialis) is really more posterior than medial and is the deepest part of this complex. It arises from the posterior part of the proximal humerus.

M. coracotriceps (anconaeus coracoideus) is the most superficial of the medial muscles of the brachium, originating from the posterior base of the coracoid flexorward (anterior) to the brachial nerves and thus bridging them. It is thus an adductor as well as an extensor of the arm.

Miner prefers to divide the triceps of *Megalobatrachus* into medial or scapulohumeral part, including what I term dorsitriceps and humerotriceps lateralis, and a coracohumeral part, including humerotriceps medialis and coracotriceps.

In *Rana* there is no separable coracoid head. A caput scapulare originates mostly from the capsule of the joint but encroaches also upon the supraglenoid region of the girdle, passing superficial to the nn. flexor brachii and pectoralis. The humeral triceps here occurs in three parts, comprising capita laterale, mediale, and profundum, the latter being merely a separable part of the caput mediale.

Innervation of the triceps is by short twigs from n. radialis, in *Necturus* usually from the postaxial branch, with the exception of the dorsitriceps of *Necturus*, which frequently receives a branch of the n. thoracodorsalis.

The brachium occupies a somewhat peculiar situation in regard to its musculature in that this segment may be oper-

ated effectively by girdle muscles inserting on the upper arm on the one hand, and antibrachial muscles arising from the distal humerus on the other. There is some indication that this may have been the basic plan, and it is difficult to decide, with both flexors and extensors, whether certain slips invaded the brachium from above or below. Not only is difficulty experienced in homologizing the triceps slips between *Necturus* and mammals, but even between the former and *Rana*. Largely because of conditions encountered in reptiles it is impossible to be certain whether the triceps heads are strictly homologous in different animals. At least, however, they are analogous.

Evidence indicates that it is a simple matter for a triceps division to change its origin from humerus to scapula via fascia or the capsule of the joint, or vice versa, but one cannot be sure that the same part of the complex has been involved or the same steps followed in diverse animals.

The conformation of the coracotriceps is particularly puzzling, arising as it does flexorward of the brachial nerves. Miner determined to his own satisfaction that in *Megalobatrachus* the upper part of the muscle is supplied by a twig of the n. coracobrachialis, indicating derivation from the latter group. If this actually be the case it would satisfactorily explain the puzzling features of the origin; but I sought such an innervation in twelve limbs of *Necturus*, under as high power of binocular magnification as was feasible, without success, and feel confident that in this animal innervation is by n. radialis alone. Furthermore stimulation of the flexor nerve components elicited no response by this division. In addition it is believed that origin of a triceps division from the coracoid is secondary, rather than fundamental. Either this head is partly derived from a group other than the triceps, as indicated by Miner's work but not by my own, or it must have migrated by a fascial band to its present origin.

The scapulotriceps is phylogenetically a somewhat unstable slip, at times arising from the scapula (*Sphenodon*), scapular fascia (most lizards), capsule of the joint (some Caudata), partly from the humerus or from the latissimus tendon (*Necturus*). Personally I am unable to abandon the idea, at present, that the condition in *Necturus* is significant. In other words it seems likely that its association with the latissimus tendon is more than fortuitous, and hence that it may actually represent a displaced part of the latissimus dorsi.

There is considerable evidence that this "dorsitriceps" division has experienced a stimulus throughout its phylogeny for reaching the area of the girdle. In Salientia (*Rana*) it does so medial to both latissimus and dorsalis scapulae tendons, and clearly has reached the girdle by a fascial rather than by a bony pathway because it passes superficial to the n. pectoralis. In Reptiles (*Iguana*) it does the same by another pathway, lateral to the latissimus.

There are two humeral heads of the triceps so uniformly that it is considered that both together in Amphibia are represented in Reptilia and Mammalia, but whether the homologue of each can separately be recognized throughout the tetrapods is a different matter. They shift around considerably, occurring as lateral and medial, or superficial and deep, so that it would be a relatively simple matter for them to exchange places.

In the case of *Rana* I am inclined to think that the triceps head termed scapulare may be considered as scapular and coracoid portions that have fused, which would naturally occur where there is no intervening obstacle, as in reptiles.

Ventral (flexor) division

Pectoral matrix (shoulder group)

M. pectoralis in *Necturus* has broad transverse origin from the ninth (?) myoseptum and sagitally from the midline. It inserts upon the anterior process of the proximal humerus. Its anterior border has practically an exactly transverse direction and it overlaps to a slight extent the major coracohumeral muscle.

In *Rana* I recognize three parts of this muscle, although four may be named. Pars cutaneous is a broad, exceedingly thin sheet from the same myoseptum as the pars abdominis, and inserts upon the skin beneath the episternum. It is of interest in being the only muscle occurring in vertebrates which simulates in essential details the m. sternalis occasionally occurring in man. Pars abdominis arises from a myoseptum well back upon the abdomen and from the border of the rectus sheath (therefore does not approach the midline), with insertion upon the distal pectoral process of the humerus. Pars sternalis is really double, with inferior origin from the xiphoid cartilage and superior origin from the sternum proper. Insertion is upon the anterior humerus medial to that of pars abdominis. The pectoralis is accordingly more extensive in *Rana* than it probably ever is in Caudata.

The pectoralis has the appearance of bearing the same relationship to the coracohumeral musculature that the latissimus does to the dorsalis scapulae, and functionally this is so. Both of the former may be considered merely as two divisions of the ventral musculature of the shoulder, but if more precise commitments be desired, then it must be supposed that both the muscle mass and nerves separated from the fused state at a time so early in phylogeny that the n. coracoideus had not yet been surrounded by the membranous girdle; else the n. pectoralis would now pierce the coracoid foramen in amphibians. Insertion was probably basically upon the capsule of the joint.

It has been supposed by some that the pectoralis of amphibians represents the entire pectoral complex of mammals. This is incorrect. It must represent either the major or minor layer of mammals and it is easily seen that the latter is the case, for the pectoral nerve in Amphibia comes off from the posterior part of the plexus and the minor layer is similarly innervated in mammals. Thus the amphibian pectoralis represents panniculus carnosus, pectoralis abdominis, and pectoralis minor of mammals.

Anterior coracoid matrix (shoulder group)

The coracoid group of muscles, termed coracohumeral to distinguish them from the coracobrachial element, occurs in two divisions as follows:

M. coracohumeralis major (supracoracoideus part) in *Necturus* is the main muscle of the coracoid, arising from practically the entire ventral surface of the cartilage and inserting upon the anterior process of the proximal humerus.

M. coracohumeralis minor (supracoracoideus part) in *Necturus* is only secondarily separable from the major division, but in other forms it is often indistinguishable. It occurs as a small slip between the major and the procoracohumeralis, from the margin of the procoracoid-coracoid notch, with insertion upon the anterior process of the proximal humerus.

In *Rana* there are three muscles in the coracoid group, as follows:

M. coracoradialis in *Rana* arises from the ventral episternal cartilage. It overlies much of the next division and underlies much of the pectoralis sternalis. It converges to a very strong tendon which passes deep down the brachium, beneath a

ligament medial to the pectoral crest of the humerus, and inserts upon the radius. This muscle constitutes the result of an effort on the part of *Rana* to manufacture a serviceable biceps of sorts.

M. coracohumeralis episternalis arises chiefly from the episternum largely beneath the coracoradial. It is a robust muscle which converges to insertion upon the medial epicondyle of the humerus.

M. coracohumeralis clavoacromialis (considered as two muscles by Ecker and Wiedersheim) in *Rana* is partly covered by the coracohumeralis episternalis and is two-headed. The smaller of these arises from the clavicle and the larger from the acromion. They fuse, and insertion is upon the pectoral ridge of the humerus.

Innervation of this group is by the coracoid nerve, piercing the coracoid foramen.

In *Megalobatrachus* Miner found that a deeper layer of the coracohumeralis was differentiated into a coracoradialis, apparently having thus developed by original connection with intermuscular fascia which eventually became tendinous. This explanation might well account for the condition in *Rana*, but it was not the situation in *Necturus*.

The history of the coracoid musculature in tetrapod ascent is one of the most interesting in myological morphology. Anchorage of the epicoracoid cartilages in the midline, which must have occurred before the recession of the coracoids was possible, allowed what may be considered as the major part of the coracohumeralis to take origin from the sternal region. This removed any need for broad coracoids and they accordingly receded to the point where they were of use only in furnishing narrow origin for brachial muscles. During this recession the coracoid nerve was liberated from its foramen and its restrictive influences. The portion of the coracohumeralis which settled to sternal origin now constitutes a part of the mammalian pectoralis. Such a derivation of any part of the pectoralis was denied by Miner, but his arguments are deemed untenable. The part concerned must be the major layer, for innervation of the minor shows clearly that the latter is derived from the orginal pectoral musculature. Hence the coracohumeral or pectoralis major layer has spread over the minor and increased in functional importance to the extent that its motor nuclei have embraced additional segments. Accordingly its nerve is now derived from posterior as well as anterior nerves of the plexus. The fact that these two muscle elements have for long functioned conjointly has doubtless been responsible for the close association of their nerves in mammals, and hence the more posterior of the nerve fibers to the pectorales major are given off in a common sheath with those of the minor, and both roots of the anterior thoracic nerve, with their connecting loop, are the result.

The more anterior or dorsal fibers of the coracohumeral appear to have had a different history. If the suprascapular nerve and musculature be not entirely unrepresented in modern Amphibia and Reptilia, and if they be ventral or flexor components, as I have somewhat reluctantly been compelled to believe, then they must have been derived from the part of the coracohumeral complex adjoining the dorsal procoracohumeral group, a thesis the essential features of which were advanced partly by Romer and by Miner. Certainly the suprascapular musculature could not have been derived from procoracohumeral (scapulohumeral) elements, as believed by Watson, and by Gregory and Camp, unless the former comprise axillary (dorsal) components

that have migrated to present position by a route medial to the scapula. Further discussion of this detail will be deferred for the contribution on the Reptilia.

Posterior coracoid matrix (shoulder group)

The morphology of this group will be discussed with the next.

M. coracobrachialis is so called to distinguish it from muscles of the coracohumeral group. In *Necturus* it lies deep to the posterior part of the coracohumeralis major, arising from the base of the coracoid and inserting upon the medial side of the humeral head.

M. gleno-antibrachialis (coracobrachialis longus, biceps) in *Necturus* arises from the posterior tuberosity of the glenoid region of the girdle, and inserts not upon the humerus strictly speaking, as stated by Wilder, but upon the capsule of the shoulder joint. The fact that it bridges the n. flexor brachii is an indication that it migrated down the arm not by a bony pathway, but via the intermuscular septum.

Miner reports for *Megalobatrachus* an additional muscle for this group, termed coracobrachialis longus, which has the appearance of being a part of the shorter coracobrachial whose insertion has settled down upon the intermuscular fascia associated with the coracoradialis. It has thus migrated distally and could by this means become a perfectly proper biceps. His biceps brachii is comparable to my gleno-antibrachialis, but it had a tendinous inscription and may be composed of different elements (as brachialis plus coracobrachialis fibers).

In *Rana* there are three muscles in this division, perfectly distinct, but all with origin from the coracoid and separate insertions upon the proximal part of the medial ridge of the humerus; hence none of this group acts as a biceps.

Innervation of this group of muscles is by the n. coracobrachialis.

Brachial-antibrachial group

The only muscle of this group to be considered in the present report comprises the following:

M. brachialis (humero-antibrachialis inferior) in *Necturus* arises from the anterior part of the proximal humerus and inserts between the antibrachial extensor and flexor groups upon the radius. There is no brachialis muscle as such in *Rana*.

Innervation is by a separate branch of n. flexor brachii.

As with the dorsal triceps division the interpretation of the brachial flexor musculature offers some difficulty. Fürbringer (1876) long ago pointed out that the biceps could not be derived from any part of the supracoracoid musculature, such as the coracoradialis of Salientia, because of the innervation of the latter. Romer expressed a similar belief, but then stated that the biceps may have arisen as a posterior part of the coracoradial, which would be an ambiguity. Romer also considered that the biceps (element ?) is absent in modern Amphibia. Certainly no element of the brachial flexors of mammals has been derived from the complex supplied in Amphibia by the coracoid nerve, except that the two groups doubtless had common origin in the pretetrapod stage. Miner believed that the biceps is a development of the coracobrachialis group,—a thesis which is quite plausible insofar as concerns Amphibia. He also contended that the brachialis had similar derivation, but of this I am doubtful. There are certainly indications that the group may have been formed not only by distal extension of shoulder components (coracobrachialis),

but also by proximal migration of elbow elements, in which case the brachialis should be a product of the latter. The coracobrachialis of mammals is undoubtedly derived from the group of the same name in Amphibia.

The conformation of the coracoradialis in *Megalobatrachus* and *Rana* are presumably efforts to supply "biceps" control of the forearm, but the innervation of the former proves that it could have taken no part in biceps formation, except as its tendon

TABLE 1

Homology of Muscles of the Shoulder

NECTURUS	RANA	MAMMAL
	Dorsal Division	
m. trapezius	trapezius	trapezius sternocleidomastoideus
absent	interscapularis	absent
m. levator scapulae mm. serrati	levator scapulae rhomboidei serrati	levator scapulae rhomboidei serratus anterior
m. latissimus dorsi	latissimus dorsi	latissimus dorsi teres major (?)
m. dorsalis scapulae	dorsalis scapulae	teres minor
m. procoracohumeralis longus	deltoideus superficialis	deltoideus
m. scapulohumeralis brevis m. procoracohumeralis brevis	scapulohumeralis brevis deltoideus profundus	post. part subscapularis
m. dorsitriceps m. coracotriceps	c. scapulare tric. c. laterale tric.	c. longum tric.
m. humerotriceps lateralis	c. mediale tric.	c. laterale tric.
m. humerotriceps medialis	c. profundum tric.	c. mediale tric.
	Ventral Division	
mm. hypobranchiales	hypobranchiales	infrahyoid mm.
m. pectoralis	pectoralis	panniculus carnosus pectoralis abdominis pectoralis minor
m. coracohumeralis major m. coracohumeralis minor	coracoradialis coracohum. episternalis coracohum. clavo-acrom.	ant. part subscapularis supraspinatus infraspinatus pectoralis major
absent	absent	subclavius
m. coracobrachialis m. gleno-antibrachialis	coracobrachiales (3)	coracobrachiales
m. brachialis	c. super. flex. carpi rad. (??)	biceps brachii (?) brachialis

The Amphibia as well as the Reptilia have clearly had some difficulty in evolving a muscle with ordinary biceps function, and the persistency with which this has been attempted indicates the importance of such a muscle to their economy. may have furnished a convenient avenue for migration of the insertion of coracobrachialis elements, as suggested by *Megalobatrachus* The gleno-antibrachialis of *Necturus* is a biceps of sorts, but whether it can be homologized with either division

of this muscle in mammals is uncertain. It is not at all improbable that elements of the absent brachialis may in *Rana* be contained in the caput superior m. flexor carpi radialis (as of Fürbringer).

There is no differentiated muscle in modern Amphibia comparable to a m. subclavius, and the derivation of the latter muscle in mammals need not concern us here.

The antibrachial and podial musculature is also outside the field of the present investigation.

In summary table 1 presents concisely the probable homologies of the shoulder musculature of *Necturus*, as illustrating modern Caudata, *Rana* for Salientia, and of the typical mammalian condition. A single interrogation mark indicates a doubt, not of the group relationship of a muscle, but whether it is justifiable to carry commitments to such a fine point. A double interrogation signifies a question whether the muscle actually has representation in other forms. Where other items of uncertainty exist they are explained in the text.

DISCUSSION OF NERVE-MUSCLE ACTION

In the present investigation there was employed a stimulating apparatus consisting of a 6 volt transformer receiving an alternating current of 60 cycles, which was then passed through a small rheostat with voltmeter. The latter was not calibrated. Unipolar stimulation was found to be more satisfactory than bipolar. The stimulating electrode was provided with the usual platinum tip, and the indifferent electrode was introduced into the oral cavity. A series of more than a dozen *Necturus* was used, and as ether anesthesia raises the threshold of action to an undesirable degree the animals were pithed between occiput and atlas. Motor stimulation was then very effective with current as low as one-tenth volt.

In spite of the fact that pithing was done with as much uniformity as possible there was extreme variability in the reflexes following this procedure. In some individuals these were of such strength as largely to simulate those of the intact animal, while in others the reflexes were sharply depressed. Individual variation in the responsiveness of muscles to the electrical stimulation of the nerves occurs, but not to a very marked extent.

The nerve impulses which react to direct electrical stimulation by effecting final muscular action are of two sorts, comprising the impulses caused by stimulating the efferent axons of the motor-neurons (motor twitch), and those caused by stimulation of the afferent elements of certain reflex arcs (reflex twitch). Just what sort of sensory fibers are concerned in the latter action is not yet known They may be proprioceptive or some other sort of sensory pathways; hence they should not as yet be designated by specific name, but for convenience in reference they will herein be termed for the time being x-afferent fibers.

In lower as in many of the higher tetrapods limb action falls naturally into two categories, one concerned with the maintenance of elevation of the body and the other with its forward propulsion. The former involves nervous posture. In reptiles and higher vertebrates the postural center lies in the red nucleus of the brain, but Amphibia and Pisces have no red nucleus. Beyond question the latter classes possess nervous posture of sorts, although it is not manifested by decerebrate rigidity and but very little is known about it. The other useful function of the fore limb in Amphibia is that of locomotion proper, the movements for

which are of a very simple pattern, with coördination of numbers of muscles. Movements other than those concerned with locomotion are minimal. Separate contraction of individual muscles is likely either impossible or, if indulged in, may be considered as the initial step of a low stimulus that would, in stronger amount, bring into action all the muscles of a particular group.

Attention should be called to the fact that such an act as locomotion is brought about by a mosaic of interdependent reflexes, controlling most of the body muscles, and the simpler the neurological organization of the animal the more completely interdependent these are. The performance of a single action instigates other sequential actions, inhibits still others, and the sum of these reflexes results in a cycle of locomotion. It is only in higher forms that one encounters any number of reflexes that are not interdependent. Thus, as far as the locomotion of vertebrates is concerned, the primitive picture is not the single reflex, but the complex reflex pattern for locomotion, and only later is this, to some extent, broken up into its component parts. It must not be forgotten, however, that when vertebrate limbs were first developing the musculature was simple and that the reflexes concerned therewith were a very small detail of the total locomotor complex. Limb reflexes developed as the importance of the trunk in progression decreased.

Primitive muscle action must, then, be considered basically as group movement. In the earliest stages of the appendages a single group movement probably included only those muscles of the same phylogenetic derivation, as the extensors, or the flexors, but not parts of both groups at the same instant. At a later stage, however, there was undoubtedly some realignment of action so that certain elements of separate muscle divisions, such as the extensors of the brachium and flexors of the antibrachium, could combine for some useful locomotor purpose. An additional step apparently involves the representation of a muscle in two different action groups.

Muscle action, and therefore nerve impulse, is not only rhythmic but basically sequential in the locomotor cycle. This may be illustrated by the sequence in muscular action as one stimulates successive brachial nerves of the dogfish, each step probably constituting a link in a reflex chain which the animal is possibly incapable of itself dividing. In higher forms the picture may be somewhat less clear cut, but the principle remains, and successive brachial nerves effect sucsive steps in the locomotor cycle of the limb concerned.

In *Necturus* the limb movements are simple and there are discernible but three distinct group actions in the pectoral limb. These can not be analyzed simply by stimulating the intact nerves, for the current would then be carried not only peripherally by the motor fibers, but also centrally by the sensory fibers of certain reflex arcs, motor axons of which may emerge from any or all of the brachial nerves. The afferent stimulation under this condition is not selective, as natura, stimuli must be, but violently inclusivel and the action resulting is not coördinated but comprises a useless gyration. Accordingly one must interrupt the afferent pathway, most easily accomplished by cutting the nerves, after tying a fine thread about each one, and then stimulating the peripheral stumps. All affected pathways will then be motor.

At a fraction of a volt the electrode elicited from the peripheral stumps of the three brachial nerves three respective

actions which go to make up a complete locomotor cycle for that limb. Stimulation was begun at about one-tenth volt and was gradually raised to one volt, during which change different actions took place as respective thresholds were attained.

Stimulation of the peripheral stump of the first brachial nerve successively brought in (1) flexion of forearm, (2) adduction and (3) protraction of the brachium. Through the stump of the second brachial nerve was secured successively (1) flexion, and slight pronation, of the manus, (2) adduction of the digits in some cases, and (3) barely perceptible retraction of the upper arm. Stronger current accentuated the latter movement and extended the forearm. When the current was applied to the stump of the third brachial nerve the appearance superficially given was that this followed through and completed the action started by the second nerve. It differed in some particulars, however. Low current through this nerve elicited (1) flexion of wrist with extension, and in some individuals abduction, of the digits, then (2) extreme retraction of the rigidly straightened arm, and finally, at one volt, (3) sufficient flexion of the forearm to bring this beneath the body.

Stated differently, stimulation of the peripheral stumps shows that the first brachial nerve bends the forearm and brings it forward partly beneath the body, so that the limb is in an advantageous position to pull and then push the body onward as the member is straightened and retracted by stimuli through the second and third nerves. The final action through the latter is slight flexion of the forearm, which is in effect the initial step toward a repetition of the entire cycle.

It must be mentioned that there is some slight individual variation in the above reactions. For instance abduction and adduction of the digits is not marked in some individuals. At times also retraction of the arm is more pronounced through the second than the third nerve. Variation of the latter sort may well be correlated with differences known to occur in the distribution of the electrically stimulable afferent fibers. Attention should also be called to the fact that the latter can not react to stimuli after the nerves are cut, so stimulation of the peripheral stumps of a cut plexus cannot present a precisely accurate picture of natural limb movement. Neither does electrical stimulation of the peripheral stump of a cut nerve take into account such inhibition of action as may occur in normal reflexes.

Coördination of limb movement implies not only that the afferent side of the reflexes concerned is in good working order, but that the pathway between the motor nuclei of the brachial segment of the cord and the limb musculature is intact. If the limb bud of a larval amphibian be transplanted several segments anterior or posterior to the normal situation the brachial nerves will make a strong effort to reach it, and will do so providing the distance is not great, as is known from the work of Detwiler and others. Movement of the limb will occur if the motor fibers of any nerve reach the limb, but coördinated movement of a useful sort will take place only providing the axons from at least one of the brachial nerves can reach the appendage (Detwiler, 1929). Such coördination, though doubtless considerably impaired, is present even if only a small branch from the last brachial nerve reaches the limb, and hence it is evident that such a branch will carry a muscle load much in excess of normal. I have verified the latter statement by extirpating large segments of one or

another of the brachial roots of *Triturus*. After a matter of some two weeks the initial impairment of function had been partly but not completely remedied, although the hiatus in the cut nerve had not been bridged. It appears likely that this was accomplished by the establishment of new reflex connections within the cord—not by a change in the motor axons or the periphery of the nerve.

In pithed *Necturus*, impact upon the sole of an anterior limb resulted in feeble movement of it and the contralateral member. Stretching was without contralateral effect.

In *Necturus* it was found that electrically stimulable afferent fibers are carried by the third and fifth spinal nerves, but not usually by the much larger fourth. This was determined as follows: In a pithed animal the fifth nerve was cut and the central stump was stimulated. Definite action resulted when the current reached a height of one and a quarter or one and a half volts, and in addition there was contraction of the ipsilateral trunk musculature of the region. The third nerve was then cut and stimulation of its central stump had the same results. In both cases the limb movement was vague and uncorrelated, probably because of the nonselectivity of the impulse. Upon the opposite side, or upon a fresh animal, the fourth nerve was then cut and its central root stimulated. In one case slight movement resulted, but in the others there was not a tremor, even in consequence of a current of five volts.

In order to determine whether the third nerve carries stimulable afferent fibers synapsing with motor elements emerging by way of the fifth nerve, and vice versa, the third and fourth spinal nerves of *Necturus* were sectioned and $1\frac{1}{4}$ volts applied to the central stump of the third. No arm movement resulted, although there was some contraction of the neighboring trunk musculature. On the opposite side of the same individual the fourth and fifth spinal nerves were similarly treated. Stimulation of the central stump of the fourth elicited fewer movements of the body and feeble adduction (flexion) of the brachium.

The above results indicate that the third and fifth spinal nerves of *Necturus* carry electrically stimulable, afferent fibers which synapse with motor neurons whose axons attain the limb by way of the fourth nerve, and with other neurons of the trunk muscles. One individual indicated that no x-afferent fibers of the third nerve synapse with efferents of the fifth, but that some x-afferents of the fifth do synapse with efferents of the third. Some individual variation in this pattern may be expected. The x-afferent fibers consistently have a higher threshold than motor fibers. It is thus seen that the stimulation of a single nerve of an intact plexus may initiate reflex action in any other nerve of the plexus.

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DELAYED IMPLANTATION AND DISCONTINUOUS DEVELOPMENT IN THE MAMMALS

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INTRODUCTION

IT HAS long been a matter of general knowledge that the rate of development of the cold-blooded vertebrates is highly variable; the rate of growth of a frog embryo, for example, being almost as exact a function of the temperature as is the speed of a chemical reaction. Even the birds have retained to some degree this power of slowing or temporarily suspending growth for the several hours or days intervening between laying and incubation. It seems, however, to be generally believed that the mammalian ovum, with its intrauterine development at practically constant temperature, has gotten completely away from the archaic habit of hesitating in its growth. At least the idea that a mammalian embryo may suddenly come to a dead stop in its development and remain quiescent for weeks or months before resuming its growth would probably come as a shock to many biologists who are not familiar with embryological literature. Yet we know at present of a dozen mammals whose period of gestation is regularly from two to six months longer than actual embryonic development, and in six of these species we have definite embryological proof that this lengthening of pregnancy is due to a quiescent period interpolated in the development. In addition to this group of species in which the quiescent period is a normal feature of development, there are several species of rodents in which development has been experimentally slowed down or temporarily stopped by various means; but at present it is uncertain as to whether this ever occurs except under the artificial conditions of the laboratory.

In spite of its unexpectedly frequent occurrence, the fact that mammalian development may be even more discontinuous than that of bird, frog, or fish has scarcely found its way into the text or reference books; certainly none of them gives any idea of the frequency with which this phenomenon is found. This cannot be blamed on any recency of the work, for seven of the cases have been known for more than forty years. It is not only the writers of texts who have overlooked this field; some of the investigators who have worked in it have been almost as negligent in ignoring the work done in other countries or on other forms. In the following pages I wish to summarize our present knowledge of the subject in the hope of making the facts more generally known and of stimulating additional work in the field.

STATEMENT OF PROBLEM

Our present interest is not in all types of lengthened gestation but only in those where the lengthening is the result of a slowing down or complete cessation of growth between the times of ovulation and of parturition. This may be made clearer by a brief review of the development of a typical mammal. Copulation and ovulation ordinarily occur within a

few hours of each other, the physiological changes which accompany ovulation determining the acceptance of breeding by the female. Fertilization thus takes place shortly after ovulation, usually in the upper part of the Fallopian tube. Cleavage goes on as the ova, or ovum, pass down the tube, and in most species these have already become blastocysts by the time they reach the uterus. As soon as the blastocysts come in contact with the uterine mucosa they attach themselves (implant) and begin the formation of the placentae. This implantation is directly controlled by a hormone secreted by the corpora lutea formed by the discharged Graafian follicles. Implantation and the formation of the placenta in nowise interrupt the growth of the ovum, the cells not concerned with implantation going ahead to form the embryo proper which continues growing in size and maturing in form throughout its intrauterine life. From ovulation to parturition the development of the embryo is a continuous, never ceasing process, one stage merging into another with the smoothness and continuity of a well-made movie film.

In sharp contrast to the foregoing is the type of development which we have called "discontinuous." In this we find the preliminary stages the same,—ovulation, fertilization, cleavage, the formation of a blastocyst. Now, however, development suddenly stops. The cells cease dividing, the blastocyst does not attach to the uterine mucosa. To continue our simile, the "movie" has suddenly become a "still," leaving a single exposure of the film projected on the screen. This stage may last for months, neither blastocyst nor uterus undergoing any discernible change. Sooner or later, however, activity resumes with the same abruptness with which it left off, the blastocyst implants, and its further history is exactly as though it had never paused in its progress from single cell to independent individual.

Discontinuous development must not be confused, as has been done by some writers, with a simple lengthening of gestation, for the latter may be due to causes other than the presence of a quiescent period. In a general way there is a rough correlation between the size of the animal and the length of gestation, and these two factors in turn largely determine the size of the young at birth. We sometimes find an animal with a gestation period unexpectedly long, as in the guinea pig where gestation lasts 63 days instead of only 20 to 40 days as in most rodents. Here, however, we find the young very large in proportion to the size of the mother, for in the guinea pig the embryo undergoes active growth during all the 63 days of pregnancy, so that the long gestation is not the result of any delay in development. Similarly we find that cases of exceptional lengthening of gestation in the human are accompanied by a proportional increase in the size of the fetus, so we may feel sure that here, too, there has been no interruption of development. It is only when an unexpectedly long gestation produces a disproportionately small fetus that we may suspect the occurrence of a delay in the growth of the embryo.

There is another type of reproductive cycle which may be mentioned here, as it offers a possibility of confusion with the type which we are to discuss. In most bats of the families Vespertilionidae and Rhinolophidae copulation takes place in the fall, the sperm are retained in the uterus through the winter, and ovulation occurs the following spring. There is thus an interval of some eight months between copulation and parturition, but this is due not to any lengthening of development but to a delay in ovulation, the actual gestation lasting only a few weeks.

These bats, then, are not included in the group of species which we are to discuss.

REVIEW OF THE LITERATURE

I shall not try to review the literature in detail, but will merely mention the more important contributions in their chronological order. It should be noted that most of the work has been based solely upon observations of the living animal, few of the investigators having seen actual embryological material. Besides the papers cited in the bibliography, there are endless references in zoological literature to the very long gestation of some of these animals. Most of these are given at second or third hand, and as they do not appreciate or discuss the possible causes of the lengthening of pregnancy, they are of little importance to our survey of the problem.

The earliest reference to lengthened gestation seems to be an article by Ziegler in 1843 on the biology of the roe-deer, *Capreolus*. Ziegler made no embryological examination. This was done in 1854 by Bischoff, who described a quiescent stage in the development of the ovum and fixed the approximate duration of this stage. In 1873 Herbst discussed the breeding time and gestation period for the European badger, *Meles*, as in the case of Ziegler without having seen any of the early stages. These were first described by Fries in 1880. Fries fixed the approximate time of implantation and naturally drew comparisons between the badger and the roe-deer.

In 1891 there appeared a remarkable paper by Lataste, in which he described, for five species of rodents, the lengthening of gestation found in lactating, pregnant animals. While he did not actually see the unimplanted blastocysts, he noted that the animals had ovulated but that the uteri showed no signs of implantation until several days after this would normally have occurred. He concluded (correctly, as we now know) that the prolongation of gestation was the result of a slowing down or cessation of development in the cleavage stages or blastocysts, and he even carried out some experiments to analyze the causes of the interruption.

From 1899 to 1902 a series of articles by Keibel brought our knowledge of the roe-deer to practical completion. Bischoff's earlier observations were confirmed, in the main, and were supplemented by the study of several score of ova from the quiescent period. Retzius, in 1900 (see also his discussion of Keibel's 1899 paper) independently studied the roe-deer and made a careful comparison with the reindeer, in which no delayed development is found. In the same year there appeared a preliminary paper by Fischer on the badger, which, however, added little to Fries' earlier findings.

The next half-dozen papers on the subject came from American zoologists. In 1910 and 1913 Daniel and King respectively confirmed for albino mouse and rat the earlier findings of Lataste. Neither author actually saw the quiescent ova. Also in 1913 Patterson described the early development of the polyembryonic armadillo, *Dasypus* (*Tatusia*) *novemcinctus*, and announced the discovery of a quiescent period immediately preceding implantation, the period being at least three weeks in duration. The free vesicles were actually recovered and described, so that the armadillo made the third species for which actual embryological evidence of the quiescent stage was available. A fourth was added in 1916 when Kirkham recovered free blastocysts from mice in which implantation was delayed by suckling. Kirkham's 1918 paper merely amplified his previous one, but in 1926 Teel advanced the problem a step by experi-

mentally inducing the delay with injections of hypophyseal extracts.

In the following year, Ashbrook and Hanson reported some breeding experiments which they had been carrying on for several years with the American marten, and which showed a very long period of gestation in this species. In that same year there appeared two articles by Prell, dealing with the long pregnancy (verlängerte Tragezeit) found in the martens. Prell's data were largely drawn from field observations, while Ashbrook and Hanson had carefully checked records of caged animals to go by. In the following three years there appeared a large number of German papers, by various authors, dealing with the observed, suspected, or possible lengthening of gestation in martens, fisher, wolverine, badgers, and bears. In none of these articles, however, were there set forth any observations of actual ova or embryos. All were based upon observations of the times of breeding and of parturition, together with a comparison with the established cases of the roe-deer and the badger. None of the investigators seems to have known of the work on the mouse and the armadillo.

In the meantime, however, other more strictly embryological investigations were in progress. Hamlett, in 1929, in a report to the Society of Zoologists, established the length of the free vesicle stage in the armadillo as being fourteen weeks; and showed that the American badger had a similar quiescent period, and probably the black bear also. In 1931 Fischer described in detail the reproductive cycle of the European badger. His material covered the whole quiescent period and was thus far more complete than that of Fries. In the following year, 1932, two articles by Hamlett completed the story of the armadillo and described material from the badger through two and one half months of the quiescent period and the actual embryogeny. Since 1930 there has also been a revival of interest in the rat and mouse, papers dealing with delayed implantation in these forms having been published by Mirskaia and Crew, Enzmann, Saphir and Pincus, Hain, and Enzmann during this period. None of these papers adds materially to the earlier findings of Lataste and Kirkham.

PRESENT KNOWLEDGE

In summarizing our present knowledge of this subject, I shall discuss one species in detail and use it as a basis of comparison for the other forms. I am choosing for this the nine-banded armadillo, partly because I am personally familiar with it, but more particularly because our knowledge of the reproductive cycle is more complete for it than for any of the other species.

Order Xenarthra

Dasypus (Tatusia) novemcinctus

Nine-banded armadillo, tatú gallinha

The papers of Patterson, Newman and Patterson, and Hamlett describe the embryology from the four-cell stage to birth. Our knowledge of the reproductive cycle (papers of Patterson and of Hamlett) is based upon the examination of over two thousand females, killed at intervals throughout the year. This species and the following one are polyembryonic, the single ovum of *novemcinctus* giving origin regularly to four embryos, that of *D. hybridus* to as many as twelve.

The nine-banded armadillo breeds, in Texas, during July. In most years it is exceptional to find a female that has ovulated during the first week of July, and most of the breeding females will have done so by the end of the month. Cleavage stages were actually recovered from

the tubes on July 9 and 16, 1928. July 15 may be taken as an average date for the time of ovulation. In a few days (5–7?) the ovum develops into an inner-cell-mass blastocyst and enters the uterus. Development ceases at this point, apparently coming to an absolute stop. The cessation of activity would seem to be inherent in the ovum, at least in this form, for vesicles washed from the lower end of the tubes seem to have attained full size and to have stopped cleavage. This would imply that the failure of implantation is not the only factor responsible for the quiescent period. The vesicle remains free in the uterine lumen until the early part of November, when the trophoblast cells become active, implantation takes place, and the differentiation of the inner cell mass begins.

During all this time the corpus luteum has been the most conspicuous feature of the ovary, one might almost say of the whole reproductive tract, for the uterus is not strikingly changed from the non-pregnant organ. Despite its large size, the corpus seems to be inactive; and this conclusion is supported both by histological study and by the completely negative results attending its removal during this period. Implantation is attended (probably preceded) by the appearance in the luteal cells of secretory droplets which give the peripheral cytoplasm a vacuolated structure. The general appearance is strikingly similar to what Corner has described for the corpora lutea of the sow. The removal of the corpus luteum after implantation is followed promptly by the abortion or resorption of the ovum. Implantation would thus seem to depend upon the secretory activity of the corpus luteum. We have at present no evidence as to the mechanism controlling the corpus, but it is doubtless correlated in some way with the pituitary.

The further development of the ovum, except for the budding of the amniotic vesicle to form the four embryonic shields, shows nothing of an unusual nature; nor can the polyembryonic feature be connected with the delay in development for it is found only in the armadillos and not in the other species which exhibit a quiescent period. The birth of the quadruplets takes place early in March (specimens from São Paulo and Matto Grosso, Brazil, show free vesicles, but I do not have enough material to fix the dates of the cycle in this region). We find accordingly in this form a total length of gestation of nearly eight months; made up of a "free-vesicle" stage of three and a half or four months and an actual development of about four months.

Dasypus hybridus

The mulita armadillo, tatú merim

Our evidence for this armadillo is indirect. Fernandez, who described the development of the embryos, failed to obtain stages prior to implantation; but he noted the presence of a corpus luteum two months earlier than he found the youngest embryos. By a comparison with *D. novemcinctus*, I believe we may safely assume that the mulita has a quiescent stage in its development. Implantation in Argentina takes place about June 1, the birth of the young in October. Having no data as to the time of breeding, we cannot fix definitely the duration of the free vesicle stage, except that it must be at least two months long. The active embryogeny lasts about four months, as in the Texas species.

Order Artiodactyla

Capreolus capreolus

European roe-deer

This is the only member of its order known to show a delay in implantation.

Our information concerning it is to be found in the papers of Ziegler, Bischoff, Retzius, and Keibel, the latter giving by far the most complete account. *Capreolus* breeds in August and implantation takes place in December. It is notable that in this species there is not a complete pause in development. A monodermic blastocyst is formed within a few days after fertilization, then during the ensuing four months the entoderm, primary amniotic cavity, and embryonic shield are slowly formed. This is unlike anything known from the other forms with delayed implantation. The fawns are born in May. The free vesicle stage lasts four months, the post-implantation development five months.

Order Carnivora

Two families and six genera apparently show the phenomenon. Definite embryological proof is available for the badgers alone.

Meles meles

European badger

Actual embryological data are given by Fries and by Fischer. *Meles* breeds late in July or early in August. Free vesicles are to be found in the uterus from that time until the latter part of January. Fischer describes them as slowly expanding, but does not describe any advance in their differentiation. Implantation takes place in January and the young are born in March. The free vesicle stage is six months long, active development two months or less.

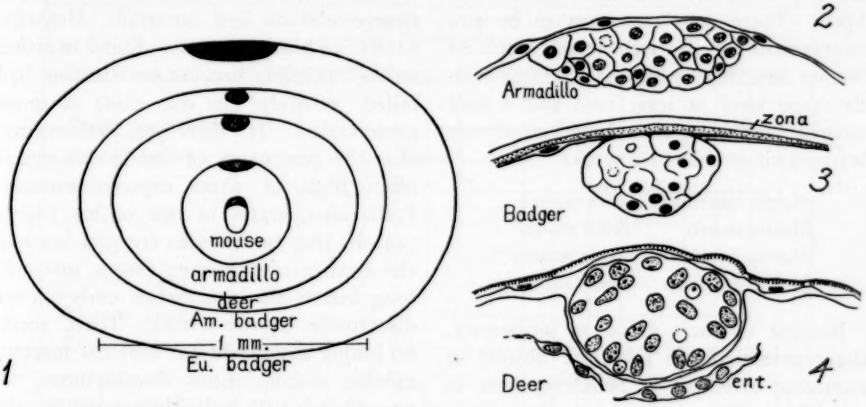


FIG. 1. OUTLINES OF BLASTOCYSTS FROM ABOUT THE MIDDLE OF THE QUIESCENT PERIOD, TO SHOW THE RELATIVE SIZES OF VESICLES AND INNER CELL MASSES IN THE VARIOUS SPECIES

The data used in constructing these are the original measurements and illustrations given in publications of Fischer, Hamlett, Keibel, Patterson, and Enzmann.

FIG. 2. DETAIL OF INNER CELL MASS OF ARMADILLO BLASTOCYST IN QUIESCENT STAGE

After Patterson. No zona present at this time. × 320.

FIG. 3. INNER CELL MASS OF BADGER (TAXIDEA) VESICLE DURING QUIESCENT PERIOD

The zona persists until just before implantation. From photomicrograph. × 320.

FIG. 4. INNER CELL MASS OF ROE-DEER FROM MIDDLE OF FREE VESICLE STAGE

Note the presence of entoderm cells at this early period, characteristic of this species. After Keibel. × 320.

Taxidea americana

American badger

This species has been studied by Hamlett. The earliest ova obtained were free

uterine blastocysts, found early in December. The species apparently breeds in August or September, judging from certain naturalists' observations, and presumably the ova would enter upon the quiescent stage a few days later. Up to this time no material has been available for the fall months. Free vesicles are found in the uterus through December, January, and the early part of February. Implantation occurs about February 15 and the young are born about the first of April. There does not seem to be any enlargement of the free vesicle such as Fischer describes in *Meles*. The free vesicle stage lasts at least two and a half months, probably six; duration of embryogenesis is about six weeks.

Martes americana	*Pine marten*
Martes martes	*Beech marten*
Martes foina	*Stone marten*
Martes zibellina	*Siberian sable*

Because of their value as fur-bearers, these animals have been the objects of numerous attempts at breeding them in captivity. The results in the past have been exceedingly conflicting; it is only recently that any carefully checked records have been available. The earliest and best of these are those of Ashbrook and Hanson on the pine marten. The animals pair during July and August, the young are born in April or May. In one case Ashbrook and Hanson isolated a female on September 4, the animal gave birth to a litter the following April. All recent observations on the Old-world species agree with those on the pine marten, except that pairing may be even earlier, in May or June.

Up to date we have not been fortunate enough to secure unimplanted vesicles from the quiescent period. We do have definite evidence, however, that we are dealing here with a true case of discontinuous development and not merely with another case of delayed ovulation such as is found in the bats. In the course of some work for the Division of Fur Resources of the Biological Survey I have had occasion this year (1935) to section the reproductive organs of two female martens from the fur-animal experiment station at Saratoga Springs. One of these animals died on August 1, the other on November 20. The ovaries of each animal show typical corpora lutea, conclusive proof that ovulation had occurred. Unfortunately, no blastocysts were found in either animal, possibly because fertilization had failed, possibly (in one case) to poor preservation. It must be remembered that the percentage of sterile matings is often high in these captive animals. Prell, also, states in one of his papers (1930b) that he has seen corpora lutea in the ovaries of a marten (stone marten?) long before the time when embryos are discernible in the uterus. There seems no longer any doubt but that the martens exhibit discontinuous development, although it is still desirable to complete the proof by actually recovering unimplanted blastocysts during the autumn months.

Euarctos americanus

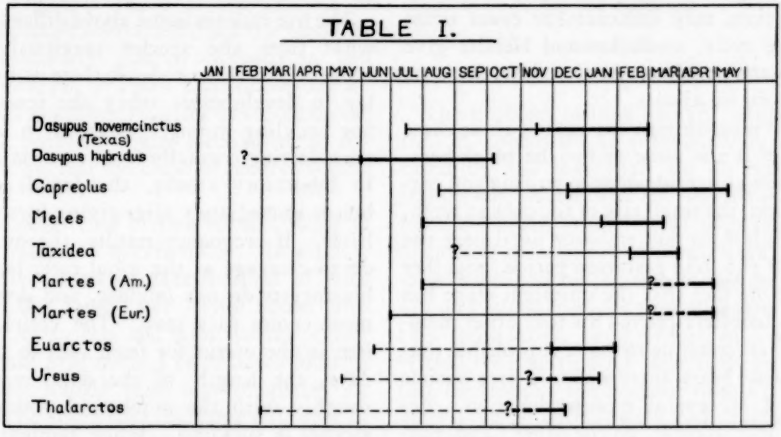
Black-bear

This seems to come the nearest of the bears to having definite proof for the quiescent period, although even here there is the possibility of another explanation for the observed facts. In the first place there is ample evidence to show that the black-bear pairs in spring or early summer, May to early July (Seton, Heller, Baker). We know also that macroscopically visible embryos are not to be found until winter. The writer has in his collection a 2-mm. embryo which was obtained through the cooperation of the Pennsylvania Board of Game Commissioners. This embryo was

taken from a bear killed December 2. Implantation therefore probably took place during the last week in November. The cubs are born during the latter part of January or the first days in February. They are absurdly small for the size of the mother, being about twelve ounces in weight or slightly larger than a new-born kitten, and plainly can have been developing for only a few weeks instead of for the half year since the mother bred.

show the latter phenomenon, while delayed implantation is of more frequent and more widespread occurrence, it seems probable that the black-bear will eventually be shown to have a free vesicle stage in its embryology. If this assumption proves correct, we would then have a cycle as follows: ovulation and cleavage in June, a quiescent period lasting until the end of November when implantation occurs, followed by six or eight weeks of embryonic

TABLE I.



EXPLANATION OF TABLE I

The table represents graphically the reproductive cycles described in the text. The first vertical mark indicates the average time when the sexes pair; this represents the time of ovulation, except possibly in the bears. The light horizontal line indicates the duration of the quiescent period (unimplanted blastocysts), the heavy line that of active development. The vertical line separating these indicates the time of implantation, the last vertical mark that of parturition.

The most obvious explanation of the facts cited above would be to assume that there exists in the black-bear an interruption of development such as has been proved to occur in other species. There is another possibility, however, which cannot be ruled out of consideration in our present state of knowledge. It may be that ovulation is delayed, and that the sperm are retained in the uterus for several months, as is known to occur in some bats. However, as only the bats are known to

development and the birth of the cubs late in January.

Ursus arctos	*European brown-bear*
Ursus horribilis	*Grizzly bear*
Thalarctos maritimus	*Polar bear*

In these species we have apparently much the same story as is found in the black-bear. The brown-bear breeds in late April, May, or June (Prell, Lönnberg) and the young are born during December, January, or February, depending appar-

ently upon the locality. The grizzly breeds somewhat later, in June or July (Seton), and the cubs are born in January. Nothing is known of what happens between pairing and parturition, except that macroscopically visible embryos are not to be found during the fall months.

The polar bear differs slightly in its time of breeding. Pairing is earlier, February to March in captive animals, and the young are born in December (Prell, Lönnberg, Heller). It is probable that conditions may influence the exact times of the cycle, as Bailey and Hendee give February or March as the time the cubs are born in Alaska.

Our reasoning in the case of these three species is the same as for the black-bear. The long interval between pairing and parturition, the small size of the cubs at birth, the failure to find embryos until near the end of the long gestation period, together with the fact that the quiescent stage has been definitely proved for four other mammals, all combine to make it probable that in these bears there exists a free vesicle period of several months duration. Actual development, on the other hand, cannot last more than six weeks or two months, judging by the size of the newborn young.

Mustela noveboracensis
Long-tailed weasel

The author has twice seen tubal blastocysts in weasels killed in northern Indiana near the end of July. At that latitude it seems unlikely that the animals could rear a second litter (the young are ordinarily born in the spring), and it is possible that these vesicles from a summer ovulation are to persist through fall and winter and develop into embryos the following spring. Warren has also raised this same question in regard to *Mustela arizonensis* in Colorado. However, we know that the closely related ferret, *Mustela putorius*, has a 40-day gestation without any delay. As in the bears and the martens, it is necessary to have material from the fall months before the question can be settled for the American weasels.

Order Rodentia

Mus musculus	*House mouse*
Mus norvegicus albinus	*White rat*
Dipodillus simoni, *Meriones shawi*, *Meriones longifrons*	*Feral African species*

The five rodents listed above differ somewhat from the species previously described. In these rodents there is no delay in development when the female is not suckling young, gestation in all of them lasting normally from 19 to 22 days. In laboratory stocks, the female often breeds immediately after giving birth to a litter. If pregnancy results, the ova undergo cleavage at the usual rate, but the blastocysts do not implant, and development comes to a stop. The vesicles lie free in the uterus for from two to fifteen days, the length of the delay varying roughly with the number of young the mother is suckling. When implantation finally takes place, growth is resumed at a normal rate and the young are born at the normal stage of development, the increased length of gestation being due wholly to the quiescent period and not to any lengthening of the time of active growth.

Some of the investigators have ascribed the failure of implantation to the drain of nourishment required by the female to maintain lactation, and have tried without much success to control the length of the delay by varying the number of young suckling. Lataste, with the same idea in mind, reasoned that any depressant of the mother's well-being should cause a similar deficiency of available nutriment, and tried with some success to cause the delay by traumatizing the reproductive tract or body wall of the mother before implanta-

tion occurred. In one case, where the female was suckling young, repeated cauterization of the thigh delayed implantation for 32 days beyond the normal time. Recently Teel attempted to control implantation by the use of hypophyseal extracts; injection of these delayed implantation for as much as six days. It would seem probable that the delay is due to some effect of the hormonal conditions prevailing during lactation rather than to such a simple thing as the amount of nourishment abstracted from the blood by the mammary glands. Repetition and amplification of Teel's experiments are indicated as a promising line of attack on the whole problem of the mechanism of implantation and the cause of the free vesicle period.

The occurrence of this induced delay in wild rodents is questionable. In a fairly extensive series of examinations of mice of various kinds, free living house mice and Norway rats and wild species of deer mice and field mice, I have never found a suckling female that was pregnant. It should be noted that the three wild species which Lataste described were bred and studied in the laboratory. I am inclined to believe that copulation immediately after parturition in the mice is a response to domestication, and is rare or lacking in free living races. In this connection we may point out that Kirkham found, in his stock of mice, that many females did not ovulate immediately after parturition and that many of those that ovulated later aborted. Only about one-fifth of his mice completed pregnancy successfully while lactating.

Order Insectivora

Sorex araneus

Common shrew

In a recent paper on the reproductive cycle of the common shrew, Brambell suggests that this animal may exhibit a delay of implantation in the lactating, pregnant female similar to what has been found in rats and mice. Brambell was led to this tentative conclusion by the large percentage of lactating females which showed free uterine vesicles as compared with the relatively smaller number of nulliparous animals containing unimplanted blastocysts. His data are admittedly inconclusive, and it will probably require observations on captive animals to settle the question.

DISCUSSION

The controlling mechanism

The problem of how delayed implantation is controlled is immensely complicated by the fact that we do not yet completely understand the mechanism of normal implantation. It is undoubtedly hormonal in action, and we do have some knowledge concerning two of the factors. We may summarize the principal points of this mechanism, as it occurs in a form without discontinuous development, before considering the possible causes of the delay. In an animal with continuous development, we find that immediately after ovulation the cells of the discharged ovarian follicle develop into a compact mass of glandular tissue known as the corpus luteum. The cells of the corpus luteum secrete into the blood a hormone which acts upon the uterus in such a way that the attachment of the ovum can be effected. That this hormone is essential for implantation is shown by the facts (1) that removal or cauterization of the corpora immediately after their formation prevents implantation; (2) that their destruction after implantation causes abortion or resorption of the ova (at least during the early part of gestation); and (3) that animals in which the corpora have been destroyed have been enabled nevertheless to go through pregnancy successfully by the daily injection of active luteal extracts.

The functioning of the corpora in turn is linked with the hypophyseal hormones and with the ovarian hormone from the Graafian follicles, but the exact way in which these hormones react on one another is not fully understood at present. Apparently the follicular hormone is antagonistic to the action of the corpus; nor can the hypophyseal hormones alone substitute for the luteal secretion, although their presence is necessary for the normal functioning of the ovary.

In the armadillo the corpus luteum grows very rapidly and attains a large size by the time the vesicle reaches the uterus. The cells of the corpus, however, do not appear to be functioning, for secretory droplets or granules are almost entirely lacking in their cytoplasm, and it is not surprising therefore that implantation does not occur. This inactive condition of the corpus luteum persists throughout the free vesicle stage. Shortly before implantation the cytoplasm of the luteal cells becomes vacuolated, with many lipoid droplets, and this presumptive evidence of secretory activity is present all through the first half of the active development. Removal of the corpus luteum during this time causes abortion; its removal during the free vesicle period causes no discernible effect on the uterus or ovum. The logical conclusion is that the failure of implantation at the usual time is due to a lack of lutein secretion, and that implantation is eventually brought about by the belated activity of the luteal cells. I have no evidence as to the nature of the stimulus that eventually starts the corpus luteum functioning. This stimulus might conceivably arise within the ovary itself, although the study of scores of ovaries has failed to reveal any change except in the corpus luteum. Personally, I think that the hypophysis is the most likely place in which to look for the immediate controlling agency. Attempts to shorten the duration of the free vesicle stage by the use of hypophyseal extracts or other hormones (luteal extracts, antuitrin S) have completely failed, however, so that our ideas must remain for the present purely speculative.

The corpora lutea in the American badger are quite large during the free vesicle stage; they do not undergo any noticeable change at implantation. The other species have not been studied from this standpoint; presumably they are more or less like the armadillo. In none of these cases do we have any cytological or histological studies of the ovary or corpus luteum.

Phylogenetic and ecologic relations

It may be of value to consider the distribution of delayed implantation among the taxonomic groups, and the conclusions that may be drawn as to the number of times this method of development has arisen in the mammals. In the first place, the fact that it is found in the roe-deer and is known *not* to occur in at least half a dozen other ungulates indicates that it has evolved independently in *Capreolus*. Similarly, the isolated position of the Xenarthra goes to show that the free vesicle stage of the nine-banded armadillo has arisen without any relationship to the other groups which show it. Since we know next to nothing of the reproductive cycles of the other Xenarthra, it is impossible to say whether it has evolved early, as a common feature of all the order, or of all the armadillos, or later after the differentiation into the present day genera had taken place.

The situation in the carnivores is somewhat more complicated. We have already pointed out our reasons for thinking that the bears exhibit this type of development. Since three genera of the family seem to agree closely in the details of their

reproductive cycles, it may be assumed for the present that this is characteristic of the family as a whole. In this case, delayed implantation must have arisen very early in the evolution of the Arctidae, before the differentiation into the various genera had been accomplished.

In the mustelids we find a different situation. Discontinuous development is found in three genera, but in several other genera (skunk, ferret, mink) the development is apparently continuous. This means that delayed implantation is an innovation later than the differentiation of the ancestral stock into the modern genera, and presumably is far more recent than the splitting of the carnivore stock into the bear and weasel groups. Delayed implantation must accordingly have been evolved independently in the two stocks, which with its occurrence in the armadillos and the roe-deer makes four separate occasions when this type of development has originated and become established in a species or group of species. Probably it should be five times, instead of four, for the badgers and martens are not closely related, and belong to different subfamilies. Their common possession of an embryonic delay is thus probably due to an independent development rather than to a common inheritance.

It seems to be impossible to discover any ecologic factor common to all the species showing delayed implantation. The German writers, unfamiliar with anything but the European literature, have assumed that this phenomenon was restricted to northern species exposed to a marked alternation of warm and cold seasons, and have leaned heavily on the supposed effects of the winter cold in bringing about the evolution of this habit. The occurrence of this type of reproduction in the genus *Dasypus* (armadillos) naturally explodes that idea, for the armadillos have evolved in South America, and it is only within the last 75 years that *D. novemcinctus* has migrated as far north even as Texas. The fact that the free vesicle stage is found in *novemcinctus* in Southern Brazil (and in *D. hybridus* in the Argentine) shows that the condition evolved in the tropics or subtropics and not during the northward migration. Also, we find other genera, sometimes closely related ones, living under the same conditions as the forms which show delayed implantation, without their reproductive cycles being affected by the environmental factors supposed to be responsible for the delay. Thus the European polecat, *Mustela putorius*, has a direct development unlike its close relatives, the martens, although it lives in the same regions and has the same habits.

Theories of the cause of discontinuous development

There have been three theories advanced to explain the reason for discontinuous development becoming the mode of reproduction in certain species. None of these, however, has dealt with all the forms, and this has invalidated most of the conclusions drawn. The three views, all advanced by German biologists, are as follows.

In his article on delayed implantation in the badger, Fries tried to explain this type of development as an adaptation for the benefit of the young. His argument was that the young need to be born as early as possible in the spring, in order that they may become old enough before the following winter to have a good chance of surviving. If the roe-deer and badger, which were the two species he was considering, had the ordinary type of development, this would require that the mating season come during the winter. Fries considered this to be an unfavorable time for the adults, due to shortage of food and to climatic

conditions, while he reasoned that in late summer or early autumn the animals should be at the height of their physical fitness. This then should be the optimum time for the rutting season. But, if the roe-deer or the badger were to become pregnant in early autumn, something had to take place to prevent the young being born during the winter, when their chance of survival would be extremely slight. In these two species this result was brought about by the interpolation of the quiescent stage in their embryology, which allowed the adults to breed when they were in best condition but postponed the birth of the young until the beginning of spring when conditions were favorable to them and at the same time did not change the duration of the actual development of the embryos. Fries thought that the bats, copulating in autumn but postponing ovulation until the following spring, were solving the same problem in a comparable manner. He does not offer any suggestion as to how this adaptive dormancy of the blastocyst may have been evolved from the usual type.

The theories of Prell as to the causes of delayed implantation are to be found in two articles (Prell, 1927b, 1930b). The second of these is in answer to Murr, whose ideas will be discussed in a subsequent paragraph. Prell's theory is based on the Pleistocene glaciation as an inciting factor; he does not seem to have known of the delay in the armadillo.

Briefly stated, his theory is as follows. Discontinuous development is found only in "old" genera, that is, genera which were in existence prior to the Pleistocene. These old groups which were exposed to the rigors of the glaciation were forced to modify their development, as the summers became so short that they found it impossible to rear their young unless these were born immediately following the long winter season. The winters were so long, however, that if the animals bred the previous summer it became necessary for pregnancy to be lengthened so that the young should not be born during the winter. In some cases this lengthening of pregnancy was brought about by the interpolation of a quiescent period. (Prell apparently fails to discriminate clearly in all cases between lengthening of actual development and discontinuous development.) The evolution of this type of development is in the nature of an adaptive response by means of natural selection preserving the favorable variations.

There are several objections that may be raised to Prell's theory. Murr has violently attacked the ages assigned to some of the genera which Prell terms "old." Without considering any specific genus or geologic period, we may reflect that after all the genera which Prell calls "new" had ancestors living in the early Tertiary just as did his "old" groups. His "new" genera are merely groups which have changed morphologically since that time. Now it is in the highest degree unlikely that the morphological stability of a genus should be frequently associated with changes in the embryology while genera which have modified their morphology never change the type of development. We should rather expect that a new environment which necessitates a profound modification of the reproductive habits would be likely to bring about the evolution of new physical characteristics also instead of preserving the archaic type. While I am not an expert paleontologist, Prell's argument gives me the impression that he has been guilty of Procrustian methods with genera and epochs in lopping and stretching facts to fit his hypothesis. In any case, the tropical armadillo with its four-month delay nullifies his whole argument.

The last of the theories regarding discontinuous development is the idea of the direct effect of temperature upon the development. For this suggestion we are indebted to Murr. His idea is that quiescence of the ovum in the mammal is due to the same cause as in the lower vertebrates, i.e., to a lowering of the surrounding temperature. In a hibernating form, such as bear or badger (which, incidentally, may not hibernate), this is due to the fall in general body temperature. In the small martens or the thin-flanked roe-deer it is due to the animal's body being too small in proportion to its surface to prevent the reproductive tract with its contained ova, or the glands of internal secretion which control the reproductive organs, from being chilled by the winter cold! Murr has tried to vary the length of gestation in the ferret by keeping the pregnant animal under various temperatures. The differences observed were very slight, two days or less, and the number of cases (six pregnancies, counting both tests and controls) wholly inadequate. It is difficult to give any serious consideration to Murr's suggestion; the fact that in all known northern forms the delay begins in summer or autumn effectively disposes of this theory, and the case of the tropical armadillo (unknown to Murr) merely emphasizes the absurdity of the idea.

From the comments made in the preceding pages it will be seen that I am not satisfied with any of the theories so far advanced in explanation of delayed implantation. To be acceptable, a theory must take into account the following points. First, that the delay is found in species living in tropical as well as in arctic or temperate regions. Secondly, in two related genera, living in the same way and in the same region, one may show the delay and the other not; for instance, *Capreolus* and the red-deer, or the martens and the ferret (polecat). Again, the delay begins in the summer, so that it cannot be due to any direct effect of cold or changing season; furthermore, the exact time varies in different species from May to August so that it becomes still more difficult to fix upon any stimulus common to all of the species. Similarly, the time of implantation varies from November to February and seemingly without reference to the length of actual gestation to follow; for in some species the young are born in May while in the bears they are born in the middle of winter so that the mother has to den up (not hibernate) for six weeks or more before the cubs are old enough and the weather warm enough for them to stir abroad.

When all the above points are considered, we can rule out of consideration any idea that discontinuous development is a direct response to some external stimulus. Fries' theory is, I believe, the most logical of the three considered, although the unnecessarily early birth of the bear's cubs and the somewhat tardy birth of the young of the roe-deer and the martens do not fit well into his hypothesis. In view of the difficulties of finding a purposive explanation of the process, I have wondered if possibly discontinuous development may not be a "useless character" such as is so common among morphological structures. The markings on the wings of night-flying moths, the arrangement of bristles on the thorax of a fly, the color of the plumage of a warbler—all these are characters of diagnostic taxonomic value and probably none of them has any definite survival value to the species concerned, but they have become fixed in the heredity of their possessors so that each one is now definitely characteristic of a certain species. Similarly, I can readily conceive that delayed implantation might have started as an alternative method, as it is today in some

rodents, and that the hormonal mechanism which governs it may have become fixed in the heredity of the species by accidental survival or by being closely associated with some other character which was of evolutionary importance until today the usual mechanism of development has been completely weeded out of the hereditary potentialities of the species concerned. I know of no definite evidence either for or against this idea, but the possibility of delayed implantation being of accidental origin rather than purposive evolution should be kept in mind in considering any theory as to the meaning of discontinuous development.

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GENETICS AND HOMOLOGY

By ALAN BOYDEN

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GENETICS, broadly defined, is that branch of biology which deals with the origin and transmission of the characteristics of organisms. Its basic principles are three, viz., the principles of Biogenesis, Heredity, and Variation. So considered, genetics is a most fundamental branch of biology and it would be expected to throw light on all the remaining branches of biological science. This should be true, for example, for taxonomy no less than for evolution. Genetics should be able to contribute knowledge of great importance to our understanding of the problems of determining relationships.

One of the most fundamental principles of taxonomy is that of homology. Definitions and usages of homology have changed since the introduction of the term by Owen in 1843. Lankester (1870) proposed to substitute the more precise term homogeny for Owen's special homology. Mivart in the same year proposed to keep Owen's general terms homology and analogy but distinguished twenty-five sub-categories most of which were more precisely defined types of homology. Others have proposed still different interpretations and definitions to be used in describing the kinds of likenesses and differences among organisms. Naturally some biologists have felt that the general term homology was so ambiguous that its value in taxonomy was very limited. But the concept is so fundamental in the analysis of relationships that it is still widely used in spite of our inability to give it precise definition. Current usage seems to be mainly that of Dendy (1916) that homology is "essential structural identity due to community of descent."

It is generally agreed, then, that homology is due to inheritance. Even Owen (1866) admitted that, "The most intelligible idea of homologous parts in such series is that they are due to inheritance." If so homology is a genetic phenomenon —an expression of gene action. Knowledge of gene action should therefore be able to illuminate and explain homology, and might even provide more precise criteria by which it might be recognized. The taxonomist turns hopefully to the geneticist for such a welcome contribution to the analysis of systematic affinities.

IS GENETIC HOMOLOGY A DELUSION?

Now as a matter of fact the geneticist has given little thought to these matters and that little has been mainly wrong. Take for example the brief discussion by Crew (1925, Chap. V, p. 152).

> The feather of the bird is not necessarily the modified scale of a reptile but may be a distinctly different characterization based upon an entirely different genotype. A certain genotype results in a certain characterization—scales, mutation—alteration—in this genotype results in a new genotype and thus leads to another characterization. The old genotype is transformed into the new but the old characterization is not transformed, it disappears and is replaced. Scales and feathers are not homologous structures—homology attempts to establish a similarity in origin and nature of structures seemingly different and is based on the assumption that during the course of evolution structures have undergone transformation yet remain fundamentally the same. In fact this conception of homologous structures cannot be accommodated by the chromosome hypothesis until it can be

experimentally demonstrated that the genes themselves can pass through a process of gradual modification.

In a similar vein and speaking for the Mendelian, Duerden (1923–24) wrote previously, using the same illustration:

> Following the older methods of comparative morphology, we can maintain that the relationship of scale and feather affords evidence that in the course of evolution the reptilian scale has grown upwards into a filament, and by a complicated system of incisions of the epidermis, due to ingrowths of the dermis, the filament has frayed out, and given rise to the many structural divisions of the feather—shaft, barbs and barbules. This is the view which has hitherto been largely accepted. . . .
>
> But no, says the Mendelian, a feather is a new structure; it is *sui generis*: it is an epidermal mutation, the result of a separate germinal change; its origin is quite apart from that of scales. . . .
>
> The delight of the morphologist in the tracing of a corresponding structure through all its many transitional stages, from its one extreme to the other, is but a delusion if a genetic relationship is the underlying idea.

If the above statements are true then indeed the taxonomist has little to hope for from genetics. But let us first be sure whether or no the above conclusions are correctly drawn. False conclusions have been drawn from an incomplete understanding of genetic facts. The real genetic basis of homology is the mechanism of gene action as it is expressed in the characteristics of the organism. Does the available knowledge of character determination through the action of genes make "genetic homology a delusion?" Absolutely not. Though a complete understanding of character determination through the interaction of genes will await the results of many critical experiments yet to be performed, still enough knowledge is available to make it quite clear that genetic knowledge does not make impossible the determination of homology and its application to the analysis of taxonomic relationships.

THE INTERACTION OF GENES

To understand this we need make but a brief examination of the mechanism of character determination by genes. How do genes condition the expression of inherited traits which may be homologous? The facts clearly indicate that the structure of any particular organ is determined by the interaction of many if not by all of the genes present in the particular individual. A great many examples could be given of which those in *Drosophila* are probably best known. The eye, the wing, and other similar organs and parts of the body are known to be conditioned each by many, many genes. According to Morgan, Bridges and Sturtevant (1925) more than 100 distinct genes have different specific effects upon the adult character of the wing. Over 70 are known to condition the eye. Even though each different pair of alleles may give monohybrid ratios when the wild type alleles of *all* the others are present, this does not mean that either of these whole organs is determined by a "single gene." It takes the wild type alleles of all these genes to give the wild type characters. Since 1925 many other genes affecting these two organs have been described and the end is not yet attained. There are doubtless many other genes yet to be discovered which will be shown to modify these characters, for it must be realized that when the wild type alleles are present in homozygous condition there is no way of detecting their action. One must await the occurrence of a mutation (or produce one with irradiation) which will give an allele with a non-wild type effect *in order to demonstrate the presence of the wild type gene*! The expression "single gene effect" is very misleading. We should think, even when considering monohybrid ratios, of $N + 1$ genes rather than of one gene alone. All these gene interact to gives

the end product, the adult organ. Some of the alleles influence color, some size, some shape, some the essential structure, but only rarely is the effect so marked as to change the organ so much that it can no longer be called an eye or a wing. In fact some at least of the characteristics of the eye or of the wing are shown as long as any eye or wing develops. Only in one case then, does the concept of genetic homology fail, and in that case all homology fails likewise for an organ cannot be homologous with no organ.

The position of Duerden and of Crew is then seen to be indefensible. However discontinuous the changes in individual genes may be the fact is that through the interaction of many genes the organ is still essentially and fundamentally similar. This is indeed the basis for the use of fundamental similarity in structure as a guide to relationships. It is not at all necessary that it be "experimentally demonstrated that the genes themselves can pass through a process of gradual modification," as Crew has stated. Nor can the fact that in higher organisms there may be a discontinuity of somas in successive generations be used to destroy the basis of genetic homology between the reptilian scale and the avian feather, for it leads to the absurd position that successive generations of corresponding feathers could not be homologous either. No! Genetic knowledge does not preclude the homology of the reptilian scale with the avian feather. Indeed genetic knowledge alone is capable of explaining the transformation of one to the other, and genetic knowledge may even give us more precise criteria of homology and of degree of taxonomic relationship as well. How can this be?

THE TEST OF ALLELISM

The genes are the specific hereditary units. The closest taxonomic relationship would then exist between individuals of identical genotype. Taxonomic relationship in general is proportional to gene identity. This is indeed the position taken by de Vries (1910) long ago, and still soundly maintained. "Systematic relationship is based on the possession of like pangens. The number of identical pangens in two species is the true measure of their relationship." The determination of taxonomic relationships and of homology could then be based on methods of testing identity in genes. Such methods are available. Of these one is crucial, i.e., the test of allelism. Alleles are often considered to be the same genes, though the different alleles of one gene may have different effects on the same character. More precisely, the genes which are allelic to each other are of the same nature and probably of the same origin and almost always affect the same characters, though Muller (1922) has reported alleles of one gene, belonging to the truncate series and occupying the same locus, which may cause "either a shortening of the wing, an eruption on the thorax, a lethal effect, or any combination of two or three of these characters." Unfortunately the test of allelism requires actual crossing of the individuals whose genes are to be examined for their identity, and crossing of species and genera is often interfered with by intersterility. Wider crosses are possible in plants than in animals, but the typical problems of homology such as homologizing the endostyle of the tunicate or *Amphioxus* with the thyroid of the vertebrate seem beyond the reach of genetic analysis at present. However, proximate methods of less certainty are capable of aiding in the determination of homologies even in species which cannot be hybridized. For example, the demonstration that particular genes in species A have certain phenotypic expressions and

linkage relations to each other and that in species B similar genes occur with corresponding linkages and expressions would make it almost certain that the genes were identical or homologous, as Weinstein (1920) and Sturtevant (1921) have pointed out.

In conclusion therefore it may be said that genetic knowledge, far from denying the existence of real genetic homology, affirms the reality of the fundamental similarity in corresponding structures due to heredity, called special homology, and gives in addition limited help in the determination of such homologies and in the analysis of taxonomic relationships generally. The further progress in knowledge of gene action will doubtless extend and clarify the nature of both homology and taxonomic relationships, but it is not likely that it will ever change the essential conclusions reached above.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

MODERN ANTHROPOLOGY VERSUS BIBLICAL STATEMENTS ON HUMAN ORIGIN. *Second Edition (Revised).*

By Sir Ambrose Fleming. Victoria Institute, London. 4d. 8½ x 5½; 28; 1935 (paper)

The Victoria Institute is a pious foundation, whose object it is "In humble faith in one Eternal God, Who created all things good, to combat the unbelief now prevalent by directing attention to the evidences of the Divine care for man that are supplied by Science, History, and Religion." To this end it arranges for addresses "from men who have themselves contributed to progress in Science and Research," such as the present paper by Sir Ambrose Fleming, the eminent radio engineer. This seems to have cheered English fundamentalists mightily and now appears in a second, revised edition. In it the author makes much of the fragmentary character of the fossil record of intermediate forms between the anthropoids and man. Yet, however fragmentary, these remains show that intermediate forms have existed. Nor does he take account of the many likenesses between man and the living anthropoids. As Huxley pointed out "the structural differences which separate man from the gorilla and the chimpanzee are not so great as those which separate the gorilla from the lower apes."

Another argument to which Sir Ambrose apparently attaches great importance seems to the reviewer sufficiently curious.

> There is another difference between the animal and "man" to which Darwinians do not seem to have attached sufficient importance, and that is the very different value of the individual life. We consider it not wrong to kill certain animals, provided it is done without unnecessary cruelty, for food or to prevent them becoming too numerous or in self-defence, but we think that the killing of a man is only justified as an equitable punishment for wilful murder or other great crime against the community. If, then, man is merely a transformed and more perfect animal, we may ask at what stage in the evolution, and why, did this peculiar attribute of sacredness in the individual life begin? If, on the other hand, he was a special creation, and not wholly a material body, the reason for this difference is not hard to see.

Has Sir Ambrose never heard of cannibals? In truth the sacredness of human life seems to be a sentiment of comparatively recent growth in human history. In many savage tribes the mores forbid the killing of a fellow tribesman but not of a stranger. Even among Christian nations, while a foreigner is relatively safe in time of peace, it is considered a highly meritorious act to take his life in time of war.

However convincing the present pamphlet may seem to a radio engineer we doubt whether it will cause any biologist to revise his conclusion that the evolutionary hypothesis is the most satisfactory explanation yet proposed of the various graded series of fossil and living forms to be found in so many parts of the plant and animal kingdoms.

CREATION AND EVOLUTION IN PRIMITIVE COSMOGONIES *and Other Pieces.*

By Sir James G. Frazer. The Macmillan

Co., New York. $3.00. 8⅝ x 5⅜; xi + 151; 1935.

The title essay of this collection traces some of the cosmological speculations of primitive tribes, which postulate on the one hand the creation of man by a god or on the other his evolution by a natural process out of lower forms of animal life.

> So hard is it for the human mind even in speculation to strike out an absolute novelty. Perhaps in the end the two hypotheses may be found to be not so irreconcilable as at first sight they appear to be. Creative evolution or evolutionary creation, like the Pre-established Harmony of Leibniz, may after all be the expression of that great cosmic process which has wrought, and is still working, all the wonders of this mysterious universe, in its majestic progress eternally moulding the forms and directing the courses alike of atoms and of stars. Thus it would not be needful to suppose that in the ordering of the universe the divine is parted from the human by an impassable gulf.

The second essay gives a brief account of "Romulus," Marie de France, and other mediaeval imitators of the fables of Phaedrus. Others deal with Gibbon at Lausanne; Sir Baldwin Spencer as Anthropologist; Canon John Roscoe, the author of *The Baganda* and other works on the Bantu peoples; and Condorcet's *Progress of the Human Mind*. The last two essays are autobiographical and contain interesting reminiscences of Frazer's boyhood and university life.

THE COMING OF MAN. *Pre-Man and Prehistoric Man.*

By George G. MacCurdy. The University Society, New York. 65 cents (paper); $1.00 (cloth); (prices slightly higher on West Coast). [Accompanying "Explanatory Reading Outline" 5 cents.] 9 x 6; x + 157; 1932.

In the earlier chapters of this book Dr. MacCurdy gives a brief account of the evolution of the primates and a more detailed description of skeletal finds of man from *Pithecanthropus* to the Cro-Magnon race and of the cultures of the Paleolithic, Mesolithic, Neolithic, Bronze and Iron Ages. In later chapters he takes up the development through the various prehistoric periods of different elements of culture, dealing with food getting; habitations; fortifications; clothing, ornament and toilet; the taming of fire; art and religion; language, music and writing; labor and industry; domestication of animals and plants; transportation; commerce; the healing art; textiles and pottery; and metallurgy. This dual arrangement involves a certain amount of repetition but it has the advantage of showing the ties that bind each epoch to those that precede and follow it. An annotated bibliography of 10 titles, a glossary and a pronouncing index are also given.

THE MAGDALENIAN SKELETON FROM CAP-BLANC IN THE FIELD MUSEUM OF NATURAL HISTORY. *University of Illinois Bulletin, Vol. 32, No. 34. Illinois Medical and Dental Monographs, Vol. 1, No. 1.*

By Gerhardt von Bonin. University of Illinois, Urbana. $1.00. 11 x 8; 76; 1935 (paper).

This is a qualitative and quantitative description of the Magdalenian skeleton uncovered in 1911 at Cap-Blanc. It possesses the distinction of being the one skeleton of its period to be under the care of an American institution. It is rather complete, although the skull was somewhat damaged and required considerable reconstruction. In summary, it is the skeleton of a young female of about 20 years of age and of about 156 cm. in stature. There are no anthropological contraindications to its being accepted as belonging to the Upper Paleolithic period. The descriptions are as complete as could be expected and whenever possible the author has included comparative data on the measurements of other skeletons of the same period.

FOSSILS. *What They Tell Us of Plants and Animals of the Past.*

By Richard S. Lull. The University Society, New York. 65 cents (paper); $1.00 (cloth); (prices slightly higher on West Coast). [Accompanying "Explanatory Reading Outline" 5 cents]. 9 x 6; viii + 114; 1935.

This introduction to the study of fossils is

thorough and at the same time briefly and interestingly written. The author discusses the nature of fossils, how they are formed, locations in which they are to be found, what the different geological strata yield, and finally the last chapters are devoted to the more highly specialized fossil forms such as the horse, elephant and mastodon, and men. There are many free hand drawings and also photographic illustrations.

THE COMING AND EVOLUTION OF LIFE. *How Living Things Have Come To Be As They Are.*

By Henry E. Crampton. The University Society, New York. 65 cents (paper); $1.00 (cloth). (Prices slightly higher on West Coast). [Accompanying "Explanatory Reading Outline" 5 cents]. 9 x 6; vi + 103; 1931.

In the first part of this book Dr. Crampton gives an excellent résumé of the evidence for organic evolution from comparative anatomy, embryology and paleontology. In the last chapter he deals with the factors of evolution, concluding that Darwin's formula of the natural selection of congenital characteristics is corroborated by all of the discoveries up to the present time. An annotated bibliography of 18 titles, a glossary and a pronouncing index are given.

REMARQUES SUR L'ÉVOLUTION DES DENTS MOLAIRES CHEZ LES MAMMIFÈRES. *Actualités Scientifiques et Industrielles* 176. *La Paléontologie et les Grands Problèmes de la Biologie Générale*, III.

By Étienne Patte. Hermann et Cie, Paris. 12 francs. 10 x 6½; 49; 1934 (paper).

The author briefly discusses the tritubercular theory of the evolution of molar teeth as modified by Gregory. The main purpose of the monograph is to answer the more recent criticisms levelled at this theory. The author therefore reviews the latest paleontologic observations; he is led to conclude that Gregory's hypothesis has been so far confirmed rather than disproven.

GENETICS

THE PRINCIPLES OF HEREDITY.

By Laurence H. Snyder. D. C. Heath and Co., Boston. $3.00. 8¾ x 5⅞; xiii + 385; 1935.

There are certain technicalities of genetics which make it a difficult subject to write about. In this book, the author seems to have bridged a number of these difficulties and, in consequence, has produced a readable and understandable text. The book is conventional in its organization, developing the cytological, breeding and biometric aspects of heredity. Human variations have been used as much as possible to illustrate the mechanism of inheritance. A welcome addition to biological textbooks.

INTRODUZIONE ALLO STUDIO DELL'EUGÈNICA (*Eredità Biologica*).

By Giuseppe Cristalli. Stabilimento Industrie Editoriali Meridionali, Naples. L. 15. 9 x 6; 219; 1934 (paper).

This is an outline of genetics which, though intended for the layman, is superior to the general run of science popularizers. As an introduction to the subject, the author gives a summary description of the gonads and of the mechanism of reproduction. There follows a satisfactory presentation and critical discussion of the various theories, from pangenesis to mutation. Throughout this work the author manifests a healthy scepticism regarding these theories and he apparently takes great pleasure in pointing out the specific weaknesses and lack of factual proofs.

HEREDITY AND VARIATION. *Continuity and Change in the Living World.*

By L. C. Dunn. The University Society, New York. 65 cents (paper); $1.00 (cloth); (Prices slightly higher on West Coast). [Accompanying "Explanatory Reading Outline" 5 cents]. 9 x 6; vi + 120; 1934.

This discussion of heredity and variation begins with a summary of Mendel's work, giving many illustrations to show the results of crossing pure and hybrid stocks.

The mechanism of heredity is treated in the usual manner. Subsequently the following topics are discussed: Variation, application of genetics to evolution, application of genetics to husbandry, inbreeding, outbreeding, applications to human society, heredity and environment, and what we inherit.

GENERAL BIOLOGY

GESTALTLEHRE DES LEBENS UND DER RASSE. *Lösung der Krise in der Medizin und Hygiene.* *By Igo Kaup. Johann Ambrosius Barth, Leipzig.* 7.50 marks. $9\frac{5}{8}$ x $6\frac{3}{4}$; vi + 154; 1935 (paper).

This arresting study presents the results of statistical, anthropometric, and experimental investigations concerning the problems of life, which the author pursued for a period of almost fifteen years. He finds that a real solution of the problem cannot be reached on the basis of a causal mechanism nor a vitalistic entelechy, but only through the concept of *Gestalt* (structure). The idea that "*Gestalt*, not matter is the fundamental principle of life' is not a new one. Its history has included the thinking of some of the greatest biologists who have ever lived. Kaup contends that just as the free order of elementary acts in "structures" and "forms" of the new physics has superseded the rigid order of Euclidian geometry and the *Weltbild* of the "classical" physics, so likewise these same concepts will prove to be the basis for a more profound understanding and a more satisfactory solution of the perplexing problems confronting the sciences of life—biology, medicine, hygiene, psychology, etc. A true biocentric world view of the realities of life will in his view thus supplement the new physical *Weltbild*. Otherwise expressed, it concerns the problem of the biological similarity of the individuals of species and races and the problem of the relationships of all the functions and stimuli of life to the totality; hence also to the problem of the body-soul unity.

Specifically, the author discusses: (1) biological similarity, morphologically and functionally considered; (2) experimental work on physiological functions and stimuli; (3) application of a *Gestalt* theory to biological, medical and hygienic problems, including such topics as *Gestalt* and heredity, *Gestalt* and hygiene inclusive of natural cures, race, domestication, and population problems, the declining birth rate, and talent and mental disease; *Gestalt* and medicine (pathology); (4) *Gestalt* and the *Geisteswissenschaften*, including the sociological and political sciences, and educational reform.

LIMNOLOGY. *By Paul S. Welch. McGraw-Hill Book Co., New York.* $5.00. 9 x $5\frac{3}{4}$; xiv + 471; 1935.

There is a very good chance that this volume will prove to be the most important new textbook published this year because it appears at a time when the science it expounds is still in a formative state, still susceptible to the influence of a critical and informative survey such as this. There is an increasing interest in the life of inland waters, both lakes and streams, and the fact that it is a field of ecology in which the quantitative methods of chemistry, physics, and physical chemistry can be used to supplement the methods of several of the biological sciences is enough to insure that still more workers will be attracted. Fundamentally, this text has three objectives: to indicate the nature of the information that is already available and by means of an excellent classified bibliography covering 54 pages to make the original data accessible; to examine the logical implications of this information; and to indicate the gaps that arise when this information is fitted into a logical system.

The text is clearly written and is free from ambiguity. There are numerous quotations, tables, definitions, and opinions, taken from research publications which are fitted into the text in a coherent way. In short, it is the kind of writing that is a tacit invitation to further observation and research, and this is the best recommendation for a textbook intended for graduate students, after all. The excellent index covers 20 pages.

AN ALMANAC FOR MODERNS.
By Donald C. Peattie. G. P. Putnam's Sons, New York. $3.00. 8 x 5; 396; 1935.
This is a collection of thumb-nail essays mainly about the natural history of the region about Washington, D. C., arranged in an around-the-year day-by-day form. The general effect is not dissimilar to what might be expected to result if one were to stew in the same pot Gilbert White, John Burroughs, Thoreau and Emerson, and then dilute the *residuum*. The author has a considerable literary talent, of which he appears to be not wholly unconscious. The book makes pleasant reading for otherwise unoccupied *lacunae* in the space-time continuum (we read it in a deck chair in mid-ocean). Its biology is generally sound and often acutely shrewd. There is also a fair amount of a simple, homely philosophy of life and living that is refreshing in these addled times.

ESSENTIALS OF TISSUE CULTURE TECHNIQUE.
By Gladys Cameron. Illustrations and Chapter on Photomicrography by C. G. Grand. Farrar and Rinehart, New York. $3.00. 9 x 6; xvi + 134; 1935.
This book should fill the important need for a succinct, readable and understandable account of the tissue culture technique. The author presents her material clearly, and commendably discusses the sort of lowly problems that puzzle the beginner, such as what instruments should be used, what are the formulae for specific solutions, how the apparatus should be arranged, etc. The book is *sensu stricto* a laboratory guide and an interesting one. There is a foreword by Robert Chambers.

GENERAL BIOLOGY. *A Textbook for College Students.*
By Henry R. Barrows. Farrar and Rinehart, New York. $3.50. $8\frac{1}{8}$ x $5\frac{1}{2}$; xvi + 624; 1935.
A textbook designed to meet the needs of a full year course in elementary college biology. It aims to enrich the cultural background of the general student as well as to furnish the basic materials required for further biological work. The volume is well illustrated and indexed. In the appendix will be found sections on the vitamins, a classification of organisms, short biographies of eminent biologists, and a list of reference books.

PARASITES ET PARASITISME.
By Pierre P. Grassé. Armand Colin, Paris. 10.50 francs (paper); 12 francs (cloth). $6\frac{7}{8}$ x $4\frac{1}{4}$; 224; 1935.
This booklet is intended for the use of medical students, physicians and others not specializing in parasitology. It is not in any sense a textbook but rather a critical discussion of the complex and diverse aspects of parasitism which the author illustrates with numerous examples. The subject matter includes chapters on the morphologic peculiarities of parasites and their significance; on the different forms of relationship between parasites and hosts; adaptation, immunity, etc. Within its limits it is a good and informative work especially useful for the general biologist, because the author has made a conspicuous effort to integrate parasitic and non-parasitic means of existence.

DEUTSCHES BIOLOGEN-HANDBUCH.
Edited by Ernst Lehmann, in collaboration with Dr. Härle, Dr. Hoss, and Dr. Mittmann. J. F. Lehmann, Munich. 3.75 marks. $7\frac{1}{2}$ x $4\frac{7}{8}$; 227; 1935 (paper).
This useful little book contains: a directory to the schools, research institutions, experiment stations, museums, and societies devoted in whole or in part to the biological sciences in Germany—including name of director and, in some cases, number of personnel; a list of journals and year-books useful to biologists; list of members, with addresses, of the Deutscher Biologen Verband. There is an index of place names.

THE SMALLEST LIVING THINGS. *Life as Revealed by the Microscope.*

By Gary N. Calkins. The University Society, New York. 65 cents (paper); $1.00 (cloth). 9 x 6; viii + 135; 1935.

This is a small but accurate survey of the Protista by a competent investigator. The author reviews the general biology, with a minimum of technicalities, of the filterable viruses, bacteria, spirochaetes, and protozoa. There are also discussions about the nature and organization of protoplasm. Drawings and photographs add to the book's value.

ON THE SEASHORE.

By L. R. Brightwell. Thomas Nelson and Sons, New York. $1.00. $7\frac{1}{4}$ x $5\frac{1}{8}$; viii + 119 + colored plate; no date [1935].

An entertaining little book written for the amateur collector, and vacationist at the seaside. The life habits of various of the simple forms of sea life are discussed. The illustrations are drawn freehand by the author. This will certainly interest the person who has had no biological training and wishes to know something of the thousands of curious animals he sees on the beach.

HOLIDAY SHORE.

By Edith M. Patch and Carroll L. Fenton. The Macmillan Co., New York. $2.00. 8 x $7\frac{1}{2}$; x + 150; 1935.

In this book Dr. Patch, the author of *Holiday Pond*, *Holiday Meadow* and *Holiday Hill* (previously reviewed in THE QUARTERLY REVIEW OF BIOLOGY), aided by Dr. Fenton's charming drawings and photographs, writes of hungry *Aster*, the starfish; *Spiny*, the green sea urchin; *Loligo*, the squid; *Limulus*, the king crab, and many other interesting animals that live in or by the sea. Delightful introduction to seashore fauna for children between four and ten.

TEXTBOOK OF BIOLOGY.

By E. R. Spratt and A. V. Spratt. University Tutorial Press, London. 9s. 6d. $7\frac{1}{4}$ x $4\frac{3}{4}$; viii + 646; 1935.

A textbook on biology by the same authors has already been noticed in this REVIEW (Vol. 10, p. 91). This book differs from the previous text only in being more elaborate and more fully illustrated. The same principles of teaching are held to as in the earlier text.

URDEUTSCHLAND. *Deutschlands Naturschutzgebiete in Wort und Bild. Lieferungen 7, 8, 9, 10.*

By Walther Schoenichen. J. Neumann. Neudamm. 2 marks each. $10\frac{3}{4}$ x $8\frac{1}{4}$; Lief. 7, 145–168 + 9 plates; Lief. 8, 169–192 + 9 plates; Lief. 9, 193–216 + 9 plates; Lief. 10, 217–240 + 8 plates; 1935 (paper).

Previous numbers of this beautifully produced and illustrated work were noticed in the June number of this volume of the REVIEW. The present numbers are devoted to geological formations in Germany.

BULLETIN DE L'ASSOCIATION DES DIPLÔMÉS DE MICROBIOLOGIE DE LA FACULTÉ DE PHARMACIE DE NANCY, *No. 10.*

Faculté de Pharmacie, Laboratoire de Microbiologie, Nancy. $9\frac{1}{2}$ x $6\frac{1}{4}$; 48; 1935 (paper).

A REVISED LIFE-ZONE MAP OF CALIFORNIA. *University of California Publications in Zoölogy, Volume 40, Number 7.*

By Joseph Grinnell. University of California Press, Berkeley. 25 cents. $10\frac{1}{4}$ x $6\frac{1}{2}$; 3 + 1 map; 1935 (paper).

HUMAN BIOLOGY

EPIDEMICS AND CROWD-DISEASES. *An Introduction to the Study of Epidemiology.*

By Major Greenwood. The Macmillan Co., New York. $5.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; 409; 1935.

In this volume the Professor of Epidemiology and Vital Statistics in the London School of Hygiene and Tropical Medicine has permitted his wide knowledge, ripe wisdom, and consummate literary talents to play over a far-flung range in the field of epidemic diseases. The treatise is not

very systematic as dull textbooks are; pedantry is completely absent from its pages; the profound scholarship that underlies it is not apparent—indeed the author's modesty has led to its painstaking concealment. It is expressly not intended as a textbook in our sense, but to inform the wider circle of persons interested "in the communal aspects of health and disease."

The book divides itself into two parts. The first, of nine chapters, treats of general principles and methods, and is essentially a short "history of ideas" about epidemic diseases. It is delightfully written, and its incisively critical attitude is its chief and best feature.

The second and longer part of the book deals with some thirteen important diseases of which cancer is one, as illustrations of the application of general principles of science, and their results. Here the technique of presentation, while still predominantly historical, has some of the characteristics of the detective story at its best.

It is impossible in the space available justly to review this important book—probably the most important book on the general principles of epidemiology, all things considered, that has appeared in a generation. We therefore attempt only to bring it to the notice of our readers with an unqualified recommendation. No student of any aspect of public health can afford not to read it, study it, and then ponder over it.

THE GEOGRAPHIC PATTERN OF MANKIND. *Student's Edition.*

By John E. Pomfret. D. Appleton-Century Co., New York. $3.00. $8\frac{3}{4}$ x $5\frac{7}{8}$; xv + 442 + 21 plates; 1935.

This is a volume of first-rate importance to every student of human biology. The preface implies that it is, directly and indirectly, an outcome of a survey course in social science given to Princeton undergraduates. Its aim is to describe in a logical and orderly way the environment of man, as he now exists on this globe. The audience for which it is primarily intended is the student of politics, economics, or sociology. It endeavors to describe such relationships as are definite between the "natural landscape" and the "cultural landscape" in various regions of the world. Taking it as a whole the author has done a superb job.

The first four chapters deal with some of the basic laws and principles of the science of geography, particularly those relating to climate. The following eleven chapters are concerned with detailed descriptions of types of environments and geographic regions.

The book is intelligently and significantly illustrated with 15 text figures and 21 plates. There is a short (much too short in fact) list of "Selected Reference Works" in lieu of bibliography, and a detailed index.

THE MUSICAL INSTRUMENTS OF THE NATIVE RACES OF SOUTH AFRICA.

By Percival R. Kirby. Oxford University Press, New York. $12.00. 10 x $7\frac{1}{2}$; 296; 1935.

Both the ethnologist and the musicologist will find a valuable addition to his source material in this extremely thorough piece of research into the music and musical instruments of the native peoples of South Africa. The investigation was aided by a generous grant from the Carnegie Corporation. In its course the author, who is professor of music in the University of Witwatersrand at Johannesburg, brought together a unique collection of the native instruments and of photographs of the natives playing them. Every choir of the orchestra is represented; there are strings, woodwinds (both flutes and reeds); brasses (though most of the horns are really "horns" rather than metal surrogates), and percussion, including xylophones. And for good measure there is the "gora," a stringed-wind instrument that is *sui generis*. Musical examples given in profusion throughout the book show that the native music is pretty primitive by our standards. But it is interesting to note that in Bechuanaland the natives go in for reed-flute ensembles, with as many as twenty or more performers in the band, with the instruments tuned in different keys.

The book is extensively documented, beautifully printed and illustrated, and well indexed. It is a contribution of first-rate importance.

After Three Centuries. *A Typical New England Family.*

By Ellsworth Huntington and Martha Ragsdale. The Williams & Wilkins Co., Baltimore. $2.50. $7\frac{3}{4}$ x $5\frac{1}{8}$; viii + 274; 1935.

This book reports the result of a very interesting attempt to study the influence of heredity on individual social and economic status. This investigation centered around the descendents of "Simon Huntington, a Puritan emigrant from eastern England, who died of smallpox at sea in 1633 and whose widow landed at Dorchester with four young sons and a daughter." They settled in New England and there the family achieved wealth and position; until more recent years they intermarried only with similar Puritan stock. The authors have traced the present descendents of this family and compared their social and economic status with that of the general population of the United States. In addition, they have selected representative Puritan surnames, certain widespread English surnames and some common non-English (Irish, German, Scandinavian, Italian and Jewish) surnames and compared the relative frequencies with which these surnames are mentioned in the different Who's Who, professional directories, etc., their ratings in Dun and Bradstreet's, and finally their incidence in the social service exchange and criminal identification lists. The results of these two orders of investigations are: (a) that the Huntington family in general occupies a higher social and economic position than the general population; (b) that on the average the Puritan surnames rank higher than the English and these higher than the non-English surnames. It is unfortunate that the authors have not tested the first findings to determine whether it is not due to sampling error. To give any definite meaning to second findings it would be necessary to place the surnames on a comparative basis relative to the time of individual immigration here.

The author's conclusions appear to be that given the right climatic conditions judicious selection and segregation will produce a superior people and to this especially he attributes the higher position of the Huntington and old Puritan families. There is a chapter entitled "What might have been" which does not belong in a book of scientific pretensions. The authors think that if after colonial days there had been no further immigration, the United States would be as a whole in better social and economic conditions! Is it to be presumed that the old Puritan families would have invented a genteel way of mining coal or laying railroad tracks or making steel rails?

The Poetry of José Mármol. *The University of Colorado Studies, Volume 22, Numbers 2 and 3.*

By Stuart Cuthbertson. University of Colorado Studies, Boulder. $2.00. 10 x $6\frac{7}{8}$; 193; 1935 (paper).

In spite of the doctrine of pan-Americanism the people of the United States for the most part know little of the culture of their Latin American neighbors. In 1865 Sarmiento wrote from Boston that "Mr. Longfellow, who is considered the most notable English poet of the epoch and who has an excellent knowledge of Spanish, lamented that he did not know any of the poetry of our country, and Mr. Ticknor, who is now perhaps the foremost Spanish critic and scholar, although he is a North American, showed the same regret. . . ."

In the seventy years since this letter was written the North American knowledge of South American literature has not increased greatly. Nevertheless a certain amount of work is being done, to which Professor Cuthbertson's scholarly study of one of the principal Argentine poets is a welcome addition.

For half a century after the establishment of Argentine independence the country suffered from the ill feeling between the urban population of Buenos Aires and the gauchos of the pampas. In 1835

General Rosas, the idol of the gauchos, of whom Darwin gives a vivid sketch in his *Voyage of the Beagle*, became dictator and proceeded to quell opposition by killing or exiling his opponents. Among the exiles was Mármol, a young intellectual who spent the twelve years until Rosas' overthrow in Montevideo and Rio de Janeiro, pursuing his multitudinous love affairs and writing invectives against his arch-enemy as well as other poems and novels. Upon Rosas' downfall he returned to Argentina and renounced literature for a political career.

In this careful study of Mármol's poetry Professor Cuthbertson concludes that most previous critics have overemphasized his political invectives, whereas his real importance is as a poet of nature, love and God. He concludes also that Mármol is far more cosmopolitan than Spanish in his literary origins and that he is one of the outstanding romanticists of Spanish-American literature.

In the bibliography of four pages Byron twice appears as "Alfred Lord Byron." Evidently Professor Cuthbertson has inadvertently confused him with another poet-peer who wrote of Sir Galahad rather than of Don Juan.

PARETO'S GENERAL SOCIOLOGY. *A Physiologist's Interpretation.*

By Lawrence J. Henderson. Harvard University Press, Cambridge. $1.25. $8\frac{3}{8}$ x $5\frac{1}{4}$; viii + 119; 1935.

This essay by one who has long been an ardent student of Pareto's theory of general sociology well serves as an introduction to the English translation of this important work which has recently appeared. Few will ever get further than these pages but the serious student will be all the more keen after pondering at length over residues, derivations, sentiments, non-logical actions, equilibrium, etc., to turn to Pareto's *Treatise*.

Briefly the *Treatise* deals with the development of a systematic method of approaching sociological inquiry. It calls for a study of the non-logical activities of individuals, for, according to Pareto, only when such a study has been made is it possible to determine the part played in human affairs by logical reasoning. About half of the present volume (59 pages) is devoted to the essay. The author uses Willard Gibbs' generalized description of a physico-chemical system, which has long served theoretical science, as an illustration of what Pareto's conceptional scheme of the social system means for sociological investigation. In the latter part of the volume will be found a series of lengthy notes (13 in all) discussing Pareto's use of terms, sources of his ideas, etc. Henderson believes that "acquaintance with Pareto's analysis of facts, with his synthesis of results, with his methods, and with some of his theorems is at present indispensable for the interpretation of a wide range of phenomena, whenever and wherever men act and react upon one another." The volume is without index.

FRONTIER FOLKWAYS.

By James G. Leyburn. Yale University Press, New Haven. $3.00. $9\frac{1}{4}$ x 6; x + 291; 1935.

A book of great interest to the general reader and to the student of social science. When people of an established order go to frontier countries in what way do they readjust themselves to their environment? How does contact with uncivilized man and the hardships which a strange and rude country thrust upon them tend to alter the mores of a group of people? Eight frontiers are discussed: The Massachusetts Bay Frontier (1629–1650); The French along the Saint Lawrence (1660–1698); New Zealand: a Frontier Founded upon a Theory (1839–1857); Portuguese Settlement Plantation Frontier in Bahia, Brazil (1549–1580); The Frontier of the Pastoral Boers in Transvaal (1835–1899); Two Frontiers in Australia (1787–1840); The Dutch Exploitative Frontier in Java; Economic Adjustments on Spanish Frontiers.

In all of these the author finds that there had been "a cultural retrogression to something primitive." In the Massachusetts colony primitive religious faith and practices appeared among the Puritans. In Canada, where there was an adaptation

of the seignorial system of France, contact with the Indians had a profound effect on the economic, political and social life of the communities. New Zealand was founded upon the theory that the only proper colonists are those who are carefully selected as to age, character and health. Definite economic principles were established for the colony before it departed from England but most of these were dropped as unworkable in the new environment. After eight years of distress the communities gradually emerged with modified mores and institutions. Each frontier had its own interesting problems which the writer skilfully depicts. The final chapter is devoted to a discussion of the study of frontiers in relation to the science of society. The volume is abundantly documented and is indexed.

THE COSMIC WAY TO TRUE CIVILIZATION THROUGH PARENTHOOD.

By George H. Donahue. [No place of publication]. $9\frac{7}{8}$ x $6\frac{7}{8}$; 68; 1934 (paper).

Dr. Donahue in his foreword relates how Socrates, while serving as an Athenian soldier "found his feet rooted to the ground in the camp of the Greek army. Thus pinned to the earth he remained for twenty-four hours in a trance,"—one wonders what his sergeant said—"the body suddenly reduced to impotence, the spirit caught up into regions whither no eye could follow it." During this trance "THE COSMIC WAY" was revealed to him. This Cosmic Way Dr. Donahue proceeds to expound. The exposition winds its devious course through the discovery of the voltaic cell, Aristotle, Priestly, Lavoisier, Dalton, Mendelejeff and the periodic table of the elements, Newton's studies in optics, Frauenhofer (*sic*), Bunsen and Kirchoff, Thomas Young, and Sir J. J. Thomson to the conclusion that

> Children must be inspired to realize that the cosmic conditions of our human existence make it impossible for anyone else to do for us what we alone can do and ought to do; that we cannot do for others what they alone can do and ought to do; that if we do not what we can do and ought to do, what we can do and ought to do remains not done; that they serve best who are best by being best; that they sacrifice most, and most inimically, who least attain; that BE and BECOME is the mandate of Religion and of Science; that Religion imparts the illimitable upward urge; that pure Science, the personal potential becoming actual in each one of us, alone can tell us how RIGHTLY to watch our footsteps; that "The Kingdom of God" is within us and is self-establishable only; that "The Golden Rule" of right conduct must become—if Democracies are to become safe for Civilization—
>
> ALL THINGS WHATSOEVER YE WOULD THAT MEN SHOULD BE TO YOU, BE YE EVEN SO TO THEM.

UN AMI DE STENDHAL, VICTOR JACQUEMONT.

By Pierre Maes. Desclée de Brouwer et Cie, Paris. 30 francs. 8 x $5\frac{1}{4}$; xii + 642 + 10 plates. no date.

In this interesting biography M. Maes tells the story of a young French naturalist of the early nineteenth century. Victor Jacquemont's father was a member of the *Conseil de l'Instruction publique* and a Tribune under the First Republic, a member of the Institute and a friend of Destutt de Tracy, Cabanis and La Fayette. Victor had the entrée of the Parisian salons where he became the friend of Stendhal and Mérimée. M. Maes proves that he collaborated in the writing of *De l'Amour*.

But, though Jacquemont might show himself a master of the theory of love, he was not equally successful in its practice. An unrequited passion led him to seek forgetfulness in America. Fresh from the salons of Paris and not a little homesick, he found few things in the New York of 1826 to please him. "The Bible," he wrote to one of his friends, "seems to me to be the scourge of America. . . .

"What I have seen in the United States has made me renounce the opinion that religious ideas can be a moral instrument useful to human societies. I have long inclined to believe it but I believe it no longer.

"Everyone here gives *all* his time to making money. . . .

"A very rich man can scarcely be convicted of anything short of murder; he buys impunity for all other offenses against society. Is it from the judges or

the juries? The public often replies: 'From both.' "

His stay in America was interrupted by the offer of the *Muséum d'Histoire Naturelle* to send him on a collecting trip to India. His experiences here were narrated in his *Voyage dans l'Inde*. Unfortunately, a promising career was cut short by his death at Bombay at the early age of 31.

The book contains a bibliography of 19 pages and an index.

AN INTRODUCTION TO THE ANTHROPOLOGY OF THE NEAR EAST IN ANCIENT AND RECENT TIMES.

By C. U. Ariëns Kappers. With a Chapter on Near Eastern Bloodgroups, by Leland W. Parr. N. V. Noord-Hollandsche Uitgeversmaatschappij, Amsterdam. $9.00. $10\frac{1}{2}$ x 7; vii + 200 + 2 folding plates; 1934.

In this monograph Professor Kappers publishes a comprehensive anthropological survey of the several population groups of the Near East and Dr. Parr contributes information on their blood groups. The senior author has gathered together numerous data on the cranial index of the skulls found in these regions and on the cephalic index of the present inhabitants. He supplements this with a summary of archeological and historical details regarding origin, migrations and intermixtures of these peoples. The anthropological data are mostly presented in the form of frequency curves, the majority of which are plurimodal. To this fact the author attributes a certain importance and appears to regard the plurimodality as the sign of specific racial mixtures. This may be so, yet the usual statistical test when applied to a few curves (continuous curve, p. 7; continuous and dotted curves, p. 9; dotted curve, p. 10) reveals that the secondary modes may be due to chance fluctuations. It is to be deplored that the author has not employed more refined statistical analysis in order to throw some light on this and other points that he makes. This publication is in every other respect an important contribution to anthropologic literature.

INSTITUTIONAL TREATMENT OF DELINQUENT BOYS. *Part 1. Treatment Programs of Five State Institutions. U. S. Department of Labor, Children's Bureau Publication No. 228. By Alida C. Bowler and Ruth S. Bloodgood. U. S. Government Printing Office, Washington.* 25 cents. $9\frac{1}{8}$ x $5\frac{7}{8}$; v + 324; 1935 (paper).

This is the first part of a report on an investigation made to determine how effective has been the work of some public institutions in educating delinquent boys. It presents a detailed description of the buildings, equipment, quality of personnel, training programs, etc., as observed in five state institutions: Whittier State School, Whittier, Calif.; Boys Vocational School, Lansing, Mich.; State Home for Boys, Jamesburg, N. J.; New York State Agricultural and Industrial School, Industry, N. Y.; Boys' Industrial School, Lancaster, O. It is surprising to note how different are the methods of training in these institutions that have theoretically one and the same objective. In this reviewer's opinion the most striking feature which emerges from this and similar reports is the incongruity between the high ideals of modern penology and the lack of the proper qualifications in the personnel of some of these institutions. The second part of this work will be awaited with interest. It will report on the subsequent career of a selected group of boys discharged from these and other institutions.

DIE RASSEN DER MENSCHHEIT.

By Hans Weinert. B. C. Teubner, Leipzig. 4.60 marks (paper); 5.60 marks (cloth). $9\frac{1}{4}$ x $6\frac{1}{4}$; vi + 139; 1935.

In the first few pages of this book the author discusses the origin of man and places the time of the splitting off of *Homo sapiens* from the chimpanzee stock in the Pliocene age. The rest of the book is a discussion of prehistoric man and the anthropological and ethnological characteristics of the present races and representative sub-races of man. The author is admittedly unable to elucidate the problem of the time, place and manner the separate forms from which the white,

negro and yellow races developed, split off from each other. The book is well written and illustrated, and there is a short bibliography of German titles.

BIRTH CONTROL IN ASIA. *A Report of a Conference Held at the London School of Hygiene and Tropical Medicine, November 24–25, 1933.*

Edited by Michael Fielding. Birth Control International Information Centre, London. 5 shillings. 8½ x 5½; 101; 1935.

At the three sessions of the Conference, at which Lord Horder presided, the following subjects were discussed: Population problems in the East; The standard of living in Asia; and The practical problems of contraception in the East. The main papers which are reported in full are by Professor A. M. Carr-Saunders (England), Dr. Rajain Kanta Das (India), Mr. V. K. Krishna Menon (India), Professor H. H. Chen (China), Mr. P. K. Wattal (India), Leonard S. Hsu and Ch'eng-Hsiu Chao (China), Dr. Helena Wright (England) and Dr. Kato (Japan). Included in the report are discussions of the papers presented by other members of the conference. The volume is indexed. Michael Fielding [a pseudonym] contributes the preface.

INTERNAL MIGRATION IN THE UNITED STATES.

By C. Warren Thornthwaite. Assisted by Helen I. Slentz. University of Pennsylvania Press, Philadelphia. $1.00. 10 x 6¾; x + 52 + 9 plates; 1934 (paper).

By utilizing population census data, certain vital statistics and school data the author is able to trace the main currents of interstate migrations in the United States and of intrastate population movements for a few selected states. The method of analysis and the graphic representations of the data are ingenious and interesting. However, there is little new in the broad facts shown: viz., that main currents of migration have been from east to west, from rural to metropolitan areas (reversed in 1929), from south to north for the negroes, and in general to wherever the economic possibilities appear more alluring.

APPLYING SCIENCE TO AGRICULTURE. *Report of Director of Oklahoma A. and M. College, Agricultural Experiment Station, for July 1, 1932, to June 30, 1934.*

By C. P. Blackwell. Oklahoma A. and M. College, Agricultural Experiment Station, Stillwater. Free. 8½ x 5¾; 318; 1934 (paper).

This report from the Oklahoma Agricultural Experiment Station is a most thorough and comprehensive survey of the experimental work done in the station for the past two years. There is a foreword by the director discussing the need for experimental work, particularly in the field of soil erosion; more than 75 per cent of Oklahoma soil is eroding badly. The report is divided into sections on soil and crop investigations, livestock investigations, poultry problems, dairy problems, chemistry in agriculture, economic and social studies, horticultural investigations, insect pest studies, and plant disease studies.

SOME THOUGHTS OF A DOCTOR.

By Frederick P. Weber. H. K. Lewis and Co., London. 6 shillings net. 8⅜ x 5⅜; 183; 1935.

These essays, by a distinguished English physician, reveal a delightful personality and the mind of one who has attained a real philosophy of life. The papers, many of which have already appeared in the medical press, cover a wide range—eugenics, free will and heredity, the gambling spirit, thoughts about thinking and dreaming, extravagance in Freudian teachings, suicide and euthanasia, Samuel Butler's theories of cell memory, nature and God, etc. Sir W. Langdon Brown contributes a foreword. The volume is without index.

FIFTY-SIX YEARS A MISSIONARY IN CHINA. *The Life of Mother St. Dominic, Helper of the Holy Souls.*

[*Anonymous*]. [*According to dust cover by Mother St. Austin, H. H. S.*] *With a Preface by Archbishop Goodier, S. J., and a Foreword by The Bishop of Nanking. Burns Oates and Washbourne, London.* 5 shillings. $7\frac{1}{4}$ x $4\frac{3}{4}$; xix + 249 + plate; 1935.

This story of the life of a missionary nun in China is concerned less with her actual experiences among the Chinese than with her saintliness. The author, a companion of the Mother for many years, writes largely of the religious thoughts and mental struggles of the nun and of her influence on the novices, giving many direct quotations from the large mass of notes and records which she left. The volume is without index but contains a few illustrations.

GEBURTENRÜCKGANG. *Mahnruf an das deutsche Volk. 3. verbesserte Auflage.*

By Richard Korherr. Süddeutsche Monatshefte, München. 1.50 marks. $9\frac{1}{2}$ x $6\frac{1}{2}$; 48; 1935 (paper).

A third revised edition of a bit of propaganda which has apparently had a wide circulation in Germany, in which the author warns of the evils consequent to a declining birth-rate by citing examples from ancient civilizations and many countries of the present time. The factors he cites as causing the present decline are various: industrialized civilization, with crowding into cities; inter-racial marriages; high suicide rates; etc. The introduction is by *Reichsführer* Himmler.

ARTS OF WEST AFRICA (*Excluding Music*). *Edited by Michael E. Sadler. Oxford University Press, New York.* $2.00. $9\frac{3}{4}$ x $7\frac{1}{4}$; xi + 98 + 32 plates; 1935.

A short illustrated book for those who are interested in the life of West Africa. The examples have been drawn from the regions between Senegal and Angola. Except for some rather general remarks on the significance and vitality of African art, the book is a series of descriptions of the illustrative plates. There are 32 plates with descriptive notes. There is also a bibliography of indigenous art in West Africa.

THE BOOK OF A LIFE. *From Generation to Generation.*

By J. C. Connell. The Ryerson Press, Toronto. $2.00. $7\frac{3}{4}$ x $5\frac{1}{4}$; 152; 1935.

The purpose of this book is to provide space to keep a yearly record of one's physical characteristics and development, mental growth, education, attainments, ailments, accidents and general important incidents of life, from birth to death. It is a kind of physical diary, with standard weight, height, and intelligence charts appended, so that anyone can see how he compares with the average.

ZEITSCHRIFT FÜR RASSENKUNDE *und ihre Nachbargebiete. 1. Band, 1. 2. 3. Hefte. 2. Band, 1. Heft.*

Edited by Egon Freiherr von Eickstedt. Ferdinand Enke, Stuttgart. 22 marks per volume of three numbers. 10 x $6\frac{7}{8}$; 112; 1935 (paper).

An excellent new journal covering various aspects of race biology—paleontology, anthropology, ethnology, genetics, eugenics, philology, geography, etc. The articles are published in English, German or French, and the list of collaborators and contributors to these first four numbers is an imposing one. The journal is worthy of a wide circulation among students of human biology, and we wish it all success.

SELECTED REFERENCES IN EDUCATION, 1934. *Supplementary Educational Monographs No. 42.*

By Department of Education, University of Chicago. University of Chicago, Chicago. 90 cents. $9\frac{3}{8}$ x $6\frac{1}{2}$; ix + 189; 1935 (paper).

The first issue of this series was noticed in Volume 9, p. 356 of this REVIEW. The reasons for and purpose of the publication were given at that time. The present volume continues the references for 1934. The personnel responsible for this volume is practically the same as that of the earlier one, so that the same accuracy is assured.

ZOÖLOGY

THE NATURAL HISTORY OF THE HERRING OF THE SOUTHERN NORTH SEA, *being The Buckland Lectures for* 1933.

By William C. Hodgson. Longmans, Green and Co., New York. $1.40. 7 x 4¾; 120; 1934.

THE HAKE AND THE HAKE FISHERY, *Being the Buckland Lectures for* 1934.

By C. F. Hickling. Edward Arnold and Co., London. 3s. 6d. net. 7 x 4¾; 142; 1935.

Francis Trevelyan Buckland, although a surgeon by profession, devoted a large part of his time to the study of animal life, especially of fish and fisheries, and was for some years an Inspector of Fisheries.

He died in 1880, and under his will he left to the nation (Great Britain) a sum of money for the endowment of a Professorship of Economic Fish Culture.

The Buckland Foundation (1926) administers the income derived from the Trust Fund, and under an Order of the Chancery Division of the High Court, dated the 5th July, 1926, three trustees have been appointed to carry into effect a scheme settled by the Court for the application of the bequest. This scheme provides for the appointment annually of a Professor of Economic Fish Culture, to be called the Buckland Professor, whose duty it shall be to deliver lectures at such places, in the United Kingdom or Ireland, as the Trustees shall from time to time determine.

The first book contains three lectures given during the autumn of 1933 at three different towns on the east coast of England. They brought to the members of the herring industry an account of recent researches on the herring and their practical application to the fishing industry. The complete life-cycle of the herring is traced in detail, with descriptions of methods of determining the age and origin of the fish and of predicting the catch.

The second book (lectures for 1934) describes the life history of the hake and the manner in which the habits of the fish affect the yield of the fishery, tells the history of the hake fishery for over a century and what is known about hake population, and discusses the effects of modern fishing methods on the future of the industry.

Both books address themselves to the fishing industry and the general public, and are simply but entertainingly written, and well illustrated. Neither book has an index.

RAISING REINDEER IN ALASKA. *U. S. Department of Agriculture Miscellaneous Publication No. 207.*

By Lawrence J. Palmer. U. S. Government Printing Office, Washington. 5 cents. 9¼ x 6; 41; 1934 (paper).

To replenish the Eskimo larder and establish a productive industry for Alaska, there were introduced into this northern region from Siberia, between the years 1891 and 1902, 1,280 reindeer. From this number have developed the present herds of approximately a million animals, and reindeer raising is now an established industry engaged in by Eskimos and whites. In 1920 the United States Bureau of Biological Survey began a scientific investigation of the production, and in 1928 was established the Reindeer Experiment Station at College, Alaska. Later two substations were set up, one at Home and another on an island in Bering Sea.

The animals are grazed on open ranges. Each herd, of about 7,000 reindeer on the average, is confined to an individual area, comprising a natural topographic unit. Each unit contains summer and winter ranges, fawning grounds, together with buildings and corrals. The reindeer are herded in much the same manner as cattle on large Western ranges except that the herders, mostly Eskimos and Lapps, follow the animals on foot and are aided by dogs. The animals are rounded up each summer to be marked and counted, and again in the fall or early winter to be butchered. Roundups may also take place in the late summer and in January or February for castrating certain of the bucks to produce steers for meat, and to reduce the number of males to a proper portion of the herd. Some cross-breeding with large selected caribou has been undertaken, resulting in heavier meat producers, better breeders, and more rugged and vigorous animals, better able to shift for themselves. The exports of meat, hides and antlers have steadily increased, and a more recent development

is the "shipping of trained reindeer to the United States each year for display purposes at Christmas time."

The pamphlet presents the latest developments in the studies being made in range and herd management, breeding, feeding, etc., and furnishes directions for carrying out the improved practises.

THE VAMPIRE BAT. *A Presentation of Undescribed Habits and Review of its History. Zoologica, Vol. 19, No. 2.*

By Raymond L. Ditmars and Arthur M. Greenhall. New York Zoological Society, New York. 30 cents. $9\frac{5}{8}$ x $6\frac{1}{4}$; 24; 1935 (paper).

In recent years the vampire bat, *Desmodus rotundus*, has been under suspicion as harboring disease organisms in its saliva. Its guilt was confirmed by the work of Drs. Herbert C. Clark and Lawrence H. Dunn of the Gorgas Memorial Laboratory in Panama, who found it to be a vector of the equine disease "murrina," fatal to horses and mules. The authors made trips to Panama and Trinidad in 1933 and 1934, carried out intensive studies in the field, and brought back specimens for further observations. In addition they have made a thorough search of the literature and thus produced a quite complete history of the vampire.

Beginning with the earliest descriptions the vampire was alleged to be a blood-sucking bat. While Darwin was the first to observe the vampire in the act of drawing blood his observations did not change this belief. It was not until 1932 that Dunn reported: "The vampire does not suck blood, as popularly believed, but takes it up with its tongue, seldom placing its mouth on the wound except when the latter is first made or when the bleeding is very slow. If the wound bleeds freely, the bat simply laps up the blood, hardly touching the tissues, while if the bleeding is scant the bat licks the wounds." With the aid of a moving picture camera Doctor Ditmars and his assistant were able to confirm this observation, and in addition to record the peculiar walking gait of the bat and its manner of stalking its prey. There are seven photographic illustrations and a bibliography of four pages.

THE DISTRIBUTION OF CERTAIN WHALES AS SHOWN BY LOGBOOK RECORDS OF AMERICAN WHALESHIPS. *Zoologica, Vol. 19, No. 1.*

By Charles H. Townsend. New York Zoological Society, New York. $1.00. $9\frac{5}{8}$ x $6\frac{1}{4}$; 50 + 4 folding maps; 1935 (paper).

From logbooks of old time whaling vessels in libraries of New England ports celebrated in the history of the whaling industry, and from privately owned logbooks, Dr. Townsend, the Director of the New York Aquarium, has compiled records of whale catches of 744 vessels (1,665 voyages). In the 22 pages of tables are given the name of the whaleship, arranged alphabetically; the date of the voyage and the catch of each voyage by species. On four large folding maps he has platted by latitude and longitude and month, the positions where the 53,877 sperm, bowhead, northern right, southern right, humpback and California gray whales were taken, the positions and extent of the whaling grounds and the seasons when they were visited during the nineteenth century, thus presenting interesting and valuable data regarding the seasonal distribution of whales and their migrations.

APPLIED ENTOMOLOGY. *An Introductory Text-Book of Insects in Their Relations to Man. Third Edition.*

By H. T. Fernald. McGraw-Hill Book Co., New York. $3.00. 9 x $5\frac{7}{8}$; x + 405; 1935.

This standard text on economic entomology has been revised largely by the addition of new material to bring the book up to date. Certain changes in the text have also been made for the purpose of expressing the facts more clearly. The author points out that the last decade has seen great advances in the field of insect control and has tried to emphasize these advances in the present edition. The book is well illustrated but, unfortunately, lacks an extended bibliography.

A MANUAL OF THE COMMON INVERTEBRATE ANIMALS *Exclusive of Insects. Thoroughly Revised Edition.*

By Henry S. Pratt. P. Blakiston's Son and Co., Philadelphia. $7.50. 9 x 6; xviii + 854; 1935.

Pratt's *Manual* is too well known to American biologists to need formal introduction. It has been our standard general taxonomic handbook for a number of years. The present edition, revised largely by systemic experts, has corrected errors which appeared in the first edition, has added new and more representative forms, has stressed the Pacific Coast fauna to a greater degree, and has added a waterproof cover (which will be welcomed by field zoölogists who invariably drop their *Manual* into a lusty stream as they struggle with the identity of some form!) A useful book.

FRIENDLY ANIMALS. *A Book of Unusual Pets. Leisure League Little Book Number 20. By Lucile Q. Mann. Illustrations by Benson B. Moore. Leisure League of America, New York.* 25 cents. 8 x 5½; 95; 1935 (paper).

This recent addition to the Leisure League Series, by the wife of the Director of the National Zoological Park, is a grand little book—so seductive, in fact, that it will be prudent not to let it fall into the hands of youngsters with a weakness for pets, if you happen to belong to the too large fraction of human beings with an aversion to queer creatures that do not understand about what Mrs. Mann quaintly calls "beautiful manners."

The book starts with the assertion that there is "scarcely an animal in the world that has not at some time been tamed and regarded with affection by its owner," and then goes on to prove, by an amazing array of examples, that this is true. It is extremely well written, coupling real literary charm with sound biology and reliable practical advice about how to make and keep pets. The line drawings that illustrate it are excellent, and have generally an appealing sort of perky quality.

Twenty-five cents cannot be better spent than on this treatise!

A GUIDE TO BIRD SONGS. *Descriptions and Diagrams of the Songs and Singing Habits of the Land Birds of Northeastern United States. By Aretas A. Saunders. D. Appleton-Century Co., New York.* $2.50. 7 x 4⅝; xvii + 285; 1935.

A unique book. The author has devised a method of recording graphically the songs of birds. By means of this and his descriptions of songs and singing habits it is possible for one to recognize to what species the song belongs without catching a glimpse of the bird. The volume, which is confined to the birds of northeastern United States, does not contain a single bird illustration. After a brief description of each species and its habits there is given a detailed account of the song with an accompanying diagram. Bird lovers will find this a fascinating and highly useful volume. It contains a key to bird songs and is indexed.

LAC AND THE INDIAN LAC RESEARCH INSTITUTE. *By Dorothy Norris, P. M. Glover and R. W. Aldis. Indian Lac Research Institute, Namkum, Ranchi, Bihar, India.* Rs. 2/8. 9¾ x 7½; iii + 53 + 12 plates; 1934.

The person who buys a can of shellac to varnish a floor is usually ignorant of the source of lac. Lac is produced in India from the resinous secretion of the lac insect, *Laccifer lacca* Kerr, a member of the Coccidae. Besides its use in the paint and varnish industry it is employed in the making of phonograph records, sealing wax, and moulded articles, as an insulating varnish in the electrical industry, and for other purposes. With the development of bakelite and other synthetic resins the Indian government, fearing that the lac industry might go the way of indigo, established a research institute to investigate improved methods of cultivation and manufacture and the development of new uses for lac. The work has included the testing of different species of host trees, their manuring and pruning, investigation of the life history of the lac insect, and control of insect enemies of it and its host trees. A list of Institute publications comprising 89 titles is given.

A PLAN FOR THE MANAGEMENT OF BROWN BEAR IN RELATION TO OTHER RESOURCES ON ADMIRALTY ISLAND, ALASKA. *U. S. Department of Agriculture Miscellaneous Publication No. 195.*

By B. F. Heintzleman and H. W. Terhune. U. S. Government Printing Office, Washington. 5 cents. $9\frac{1}{8}$ x $5\frac{7}{8}$; 20; 1934 (paper).

The management of bears of Admiralty Island in Tongass National Forest under the plan set forth in this pamphlet is a coöperative project between the Alaska regional office of the Forest Service and the Alaska Game Commission, both functioning under the Department of Agriculture. It aims to coördinate the management of the bears with timber management activities in connection with the prospective pulp and paper-making development in southeastern Alaska. The essential details of the plan are: To maintain the present bear population of the island by limiting the kill to not exceeding the yearly increase, by seasonal closings of parts of the island to bear hunting and establishing semi-permanent closed areas; to regulate the location of trails, portages, cabins, logging camps and hunting lodges; and to establish a launch patrol around the island during the active salmon spawning period when the bears are congregated at the salmon streams.

BIRD PORTRAITS IN COLOR. *Two Hundred Ninety-Five North American Species.*

Text by Thomas S. Roberts; Illustrations by Allan Brooks, George M. Sutton, Walter A. Weber, Francis L. Jaques, Walter J. Breckenridge and Louis Agassiz Fuertes. University of Minnesota Press, Minneapolis. $3.50. 11 x $8\frac{1}{4}$; vi + 90 plates and 90 pages description of plates + 14 pages index; 1934.

This is essentially a reprint of the plates which illustrated *The Birds of Minnesota* with new descriptive text. The descriptions have been made general in application so that they may serve anywhere within the range of the species. Most of the birds of the northern United States and Canada east of the Rocky Mountains are included. A brief description of each bird depicted is given, including its size and distinguishing marks, its range, its most interesting habits, the type and location of its nest and the color of its eggs. The index includes the popular names in common use in various parts of the country as well as the official and scientific names.

THE HAWKS OF NORTH AMERICA. *Their Field Identification and Feeding Habits.*

By John B. May. National Association of Audubon Societies, New York. $1.25. $9\frac{3}{4}$ x $7\frac{1}{2}$; xxxiv + 140; 1935.

This book has been issued as part of an intensive educational campaign by the Audubon societies in behalf of the hawk. The author, widely known as an economic ornithologist, has produced an exceedingly attractive volume. The content is limited to means of identification of the various species (in this the excellent illustrations will be found most useful) and to their food habits. Maps are included showing the geographical occurrence of the species, and in some cases both present and former breeding ranges and wintering areas are shown. There is an appendix on State laws relating to hawks and a lengthy reference bibliography is given but the volume is not indexed.

A SECOND LIST OF ANTILLEAN REPTILES AND AMPHIBIANS. *Zoologica. Scientific Contributions of the New York Zoological Society, Vol. 19, Number 3.*

By Thomas Barbour. New York Zoological Society, The Zoological Park, N. Y. 50 cents. $9\frac{1}{2}$ x $6\frac{1}{2}$; 64; 1935 (paper).

In December, 1930, Barbour published a list of the Antillean reptiles and amphibians in *Zoologica*. Since that time so many additional discoveries have been made that the first list is out of date. The large number of requests for the list having indicated its usefulness the author has prepared a second list of the reptiles and amphibians of the Antilles. The Antilles considered faunistically comprise the West Indian Islands, except Trinidad, Tobago and the islands off the coast of South and Central America.

THE REPTILES OF CHINA. *Turtles, Crocodilians, Snakes, Lizards. Natural History of Central Asia, Volume 10.*

By Clifford H. Pope. American Museum of Natural History, New York. $10.00. 11 x 8¼; lii + 604 + 27 plates and folding table; 1935.

This book reports studies of Chinese reptiles made by field workers of the American Museum of Natural History. The turtles, snakes, crocodiles, and lizards are all treated in detail. The material is largely taxonomic in nature, the forms being considered species by species. The annotations for each species include such data as the taxonomic history of the form; a morphological description; notes on habits and habitats; source of the material examined, and specific remarks. Taxonomic keys, as well as illustrations, serve to facilitate determinations. An important contribution to the herpetology of the world.

ONCHOCERCIASIS *with Special Reference to the Central American Form of the Disease. Contributions from the Department of Tropical Medicine and the Institute for Tropical Biology and Medicine, No. 6.*

By Richard P. Strong, Jack H. Sandground, Joseph C. Bequaert and Miguel M. Ochoa. Harvard University Press, Cambridge. $4.00 (paper); $5.00 (cloth). 10⅜ x 7½; xiv + 234 + 6 plates + map; 1934.

In the African and Central American tropics there is a disease known as onchocerciasis which is caused by a filarian worm, *Onchocerca caecutiens*. Little of an exact nature has been known about the clinical and biological aspects of this malady until the present report, which summarizes much material on the pathology, etiology, epidemiology and general biology of the disease. The authors show that this nematode is associated with the production of subcutaneous tumors, as well as other pathological lesions, in the head and thorax region. The worms are transmitted by the fly, *Simulium*. This is an important contribution to medical helminthology.

THE EUROPEAN CORN BORER AND ITS CONTROLLING FACTORS IN THE ORIENT. *U. S. Department of Agriculture Technical Bulletin No. 455.*

By Charles A. Clark. U. S. Government Printing Office, Washington. 5 cents. 9¼ x 6; 38; 1934 (paper).

This bulletin is based on the results of an investigation of the European corn borer and its parasites in Japan, Manchuria, Chosen and Taiwan. It was found that no one parasite attacked *P. nubilalis*, and therefore the authors recommend that the proper policy with regard to "parasite introduction against *Pyrausta nubilalis*, was to collect and ship all the primary parasite species obtainable without regard to the stage of the host which they attacked." More intensive investigations of the problem are needed.

THE ANIMAL WORLD. *Animal Life of Our Earth.*

By James G. Needham. The University Society, New York. 65 cents (paper); $1.00 (cloth); (prices slightly higher on West Coast). [Accompanying "Explanatory Reading Outline" 5 cents]. 9 x 6; vi + 116; 1931.

The author traces the history of man's domestication and use of animals from remote times to the present. The last chapters are devoted to a discussion of many different subjects, such as metamorphosis, migrations, methods of defense of animals, hibernation, animal societies and castes, and finally the "world of infinitely small things" as revealed by the microscope. Although such a wide variety of subjects is covered there is little confusion and the book serves as an excellent orientation to the animal world.

THE WATERFOWL FLYWAYS OF NORTH AMERICA. *U. S. Department of Agriculture Circular No. 342.*

By Frederick C. Lincoln. U. S. Government Printing Office, Washington. 5 cents. 9¼ x 6; 12; 1935 (paper).

There are four distinct flyways followed in the spring and fall migrations of the waterfowl. The terms given to them

are Atlantic, Mississippi, Central and Pacific. The most rapid decrease in recent years has been in the Central and Pacific Coast areas, resulting from drought, agricultural activities and overshooting. The decrease has been so rapid that a complete suspension of shooting privileges for a time may become necessary on all of the flyways.

BIRDS IN BRITAIN TO-DAY.

By Geoffrey C. S. Ingram and H. Morrey Salmon. Ivor Nicholson and Watson, London. 12s. 6d. net. $9\frac{1}{2}$ x $7\frac{1}{4}$; x + 145 + 51 plates; 1934.

This book is an interesting natural history study of British birds based upon the personal experiences and bibliographic researches of the authors. The volume takes up the birds taxonomically and gives descriptive data about the forms themselves and their habits and habitats. There are some 66 excellent illustrations supplementing the text.

DROSOPHILA MELANOGASTER MEIG. *Eine Einführung in den Bau und die Entwicklung.*

By Eduard H. Strasburger. Julius Springer, Berlin. 6.90 marks. $9\frac{3}{4}$ x $6\frac{1}{2}$; iv + 60; 1935 (paper).

This introduction to the anatomy and development of *Drosophila melanogaster* contains six sections. Section I deals with feeding, raising, general care, and different histological techniques to be used. Section II discusses larvae from histological and anatomical aspects; Section III, pupae; Section IV, imagoes; Section V, the embryological development; Section VI, bibliography. The book is intended as a general laboratory guide.

LEÇONS DE ZOOLOGIE. *Annélides. Actualités Scientifiques et Industrielles 196.*

By M. Prenant. Hermann et Cie, Paris. 16 francs. 10 x $6\frac{1}{2}$; 95; 1935 (paper).

In this book the general biology of the phylum Annelida is reviewed in a conventional way by discussing the typeforms *Polygordius*, *Nereis*, *Lumbricus* and *Hirudo* briefly and somewhat inadequately. The reviewer was not greatly impressed by the book. It is interesting to note that the echiurid worms are placed with the annelids.

LEÇONS DE ZOOLOGIE ET BIOLOGIE GÉNÉRALE. VII. *Vertébrés Supérieurs (Oiseaux et Mammifères). Actualités Scientifiques et Industrielles, 207.*

By Georges Bohn. Hermann et Cie, Paris. 18 francs. 10 x $6\frac{1}{2}$; 131; 1935 (paper).

This is the seventh in a series of outlines of zoology and general biology. It treats of the embryology, anatomy and physiology of birds and mammals. The descriptions are schematic but from them the student should be able to learn the essentials and the method of approach to a more complete study of the subject. Included is also a well written summary of Lamarck's and Darwin's theories of evolution.

A REVISIONAL STUDY OF THE GENUS SCOLYTUS GEOFFROY (ECCOPTOGASTER HERBST) IN NORTH AMERICA. *U. S. Department of Agriculture Technical Bulletin No. 431.*

By M. W. Blackman. U. S. Government Printing Office, Washington. 5 cents. $9\frac{1}{4}$ x 6; 31; 1934 (paper).

This bulletin attempts to clear up the uncertainty which has existed in the past about the identity of several of Leconte's species of *Scolytus*. By far the major portion of the bulletin is devoted to descriptions of the North American species. Twenty-three different species are discussed in detail.

DEVELOPMENT OF THE SWINE NEMATODE STRONGYLOIDES RANSOMI AND THE BEHAVIOR OF ITS INFECTIVE LARVAE. *U. S. Department of Agriculture Technical Bulletin No. 437.*

By John T. Lucker. U. S. Government Printing Office, Washington. 5 cents. 9 x $5\frac{3}{4}$; 30; 1934 (paper).

It is shown in this pamphlet that the

infective larvae of the swine nematode *Strongyloides ransomi* enter their host either through the skin or else through the mouth. Certain control measures for killing the larvae are recommended.

THE REPRODUCTIVE HABITS OF THE COMMON CATFISH, AMEIURUS NEBULOSUS (LE SUEUR), WITH A DISCUSSION OF THEIR SIGNIFICANCE IN ONTOGENY AND PHYLOGENY. *Zoologica, Volume 19, Number 4.*

By C. M. Breder, Jr. New York Zoological Society, The Zoological Park, New York. 35 cents. 9½ x 6⅜; 42; 1935 (paper).

MAMMALS COLLECTED BY T. T. AND E. B. MCCABE IN THE BOWRON LAKE REGION OF BRITISH COLUMBIA. *University of California Publications in Zoology, Volume 40, Number 9.*

By E. Raymond Hall. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 24; 1934 (paper).

GEOGRAPHIC DISTRIBUTION OF POCKET GOPHERS (GENUS THOMOMYS) IN NEVADA. *University of California Publications in Zoology, Volume 40, Number 10.*

By E. Raymond Hall and William B. Davis. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 16; 1935 (paper).

CILIATES FROM BOS GAURUS H. SMITH. *University of California Publications in Zoology, Volume 39, Number 17.*

By C. A. Kofoid and John F. Christenson. University of California Press, Berkeley. 75 cents. 10¼ x 6½; 52 + 5 plates; 1934 (paper).

A COMPARISON OF THE DISTRIBUTION OF THE INTESTINAL PROTOZOA OF THE NORWAY RAT, WOOD RAT, AND GUINEA PIG, WITH REFERENCE TO THE HYDROGEN ION CONCENTRATIONS AS DETERMINED BY THE GLASS ELECTRODE. *University of California Publications in Zoology, Volume 41, No. 1.*

By C. A. Kofoid, E. McNeil, and A. E. Bonestell. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 8; 1935 (paper).

THE CYCLE OF TRYPANOSOMA CRUZI IN TISSUE CULTURE OF EMBRYONIC HEART MUSCLE. *University of California Publications in Zoology, Volume 41, Number 3.*

By C. A. Kofoid, F. D. Wood, and E. McNeil. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 2; 1935 (paper).

BOTANY

A TEXTBOOK OF GENERAL BOTANY. *Third Edition.*

By Gilbert M. Smith, James B. Overton, Edward M. Gilbert, Rollin H. Denniston, George S. Bryan and Charles E. Allen. The Macmillan Co., New York. $3.50. 8½ x 5⅝; x + 574 + 4 plates; 1935.

A LABORATORY MANUAL OF GENERAL BOTANY. *Revised Edition.*

By Emma L. Fisk and Ruth M. Addoms. The Macmillan Co., New York. $1.00. 8½ x 5½; x + 137; 1935.

As textbook revisions go, this standard college textbook has been subjected to drastic changes since the appearance of the second edition seven years ago. New chapters have been added and others have been shifted about to create a more logical sequence of topics; the results of recent research have been incorporated; and the text has been rewritten generally. The quality of the illustrations has been improved by redrawing some, enlarging many, and by a general improvement in the reproduction of both drawings and photographs. Many substitutions have been made to good advantage. The last three-fifths of the book, dealing with the various divisions of the plant kingdom, have been somewhat less changed than the rest, but here, too, improvement is evident. It is a first-rate textbook. There is an excellent index.

This excellent laboratory manual was written to accompany the foregoing *Textbook of General Botany*, though of course it has other uses. "The manual as it stands is suitable for use in a year course involving four or five hours laboratory work per week." Care has been exercised that the student should understand the significance of the observations he is called upon to make and the directions are reasonably complete. An appendix contains valuable notes for the laboratory instructor on the sources of material, and the formulae for reagents and culture solutions are clearly given.

PRIMITIVE LAND PLANTS *also known as The Archegoniatae.*

By F. O. Bower. The Macmillan Co., New York. $8.00. $8\frac{3}{4}$ x $5\frac{7}{8}$; xiv + 658; 1935.

In summary, Bower says:

> The chief object of this work has not been to trace phyletic relations between Archegoniate Plants; though those who will may be interested to use the data here supplied in that way. It has rather been by examining them comparatively, to visualise the Methods of Advance which these primitive Land-Plants appear to have followed in their evolution. The primary source of evidence has been the study of the living Archegoniatae; but the reasoning based on these has been checked by reference to palaeontological facts. This mode of enquiry should lead to more valuable results in the organographic study of Land-Plants at large than any mere search for phyletic schemes.

The fundamental nature of this work does not need to be pointed out to students of plant morphology. It represents a continuation of the line of work that led to the publication of *The Origin of a Land Flora* in 1908 rather than a revision of that book. The important literature citations are listed in each chapter and there is a good index.

RESPONSES OF STRAWBERRY VARIETIES AND SPECIES TO DURATION OF THE DAILY LIGHT PERIOD. *U. S. Department of Agriculture Technical Bulletin No. 453.*

By George M. Darrow and George F. Waldo. U. S. Government Printing Office, Washington. 5 cents. 9 x $5\frac{3}{4}$; 31; 1934 (paper).

The studies described in this bulletin were conducted to ascertain the influence of light upon the regional adaptation of different varieties of strawberries. Among the conclusions reached are:

> Southern varieties grow under short days at relatively low-growing temperatures and need little rest or no dormant period.
>
> Northern varieties grow very little under short days and, if first exposed to short daily light periods, require a low-temperature dormant period to break their rest period.
>
> The response of varieties to light conditions during October, November, and December is considered indicative of their regional adaptation. Varieties adapted to southern States produce relatively large leaves with long petioles, while northern varieties grow little or not at all during these months.

INFLUENCE OF SPACING AND TIME OF PLANTING ON THE YIELD AND SIZE OF THE PORTO RICO SWEETPOTATO. *U. S. Department of Agriculture Circular No. 327.*

By J. H. Beattie, Victor R. Boswell and E. E. Hall. U. S. Government Printing Office, Washington. 5 cents. 9 x $5\frac{3}{4}$; 10; 1934 (paper).

Results of experiments conducted with the Porto Rico sweet potato at the Pee Dee Experiment Station at Florence, S. C., include:

> Spacings of 6, 9, 12, and 15 inches between plants of the Porto Rico sweetpotato in 4-foot rows showed that the closer spacings decreased the proportion and yield of jumbo size roots and that the 6-inch spacing increased the proportion and yield of No. 2 sweetpotatoes over the 9, 12, and 15-inch spacings. These spacings did not appreciably affect the proportion or yield of No. 1 or the total yield.
>
> There was a consistent decrease in yield of No. 1 sweetpotatoes as the planting date was delayed.
>
> These investigations have shown that closer spacing is better than delayed planting to reduce the proportion of jumbos, since closer spacing within proper limits does not reduce yields of marketable roots or the total yield.

AN INTRODUCTION TO PLANT LIFE.

By Carl L. Wilson and Julia M. Haber. Henry Holt and Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xiv + 493; 1935.

This textbook is intended for colleges offering half-year courses in botany for students who are not expected to continue biological work. Accordingly, the subject matter is for the most part limited to the topics that make the readiest appeal to the interest of students, the structure and properties of economic plants, the relation of plants to man, and the similarities and differences of plant and animal structure and function. The discussions of life histories have been greatly abbreviated, compared with other texts, and in general the kind of botanical knowledge that is based on microscopical observation is presented very briefly. There are a large number of illustrations, most of them photographs; bibliographies of reference books follow each chapter, and there is a good index.

GYMNOSPERMS. *Structure and Evolution.*

By Charles J. Chamberlain. University of Chicago Press, Chicago. $4.50. 9 x 6; xi + 484; 1935.

This is a revision of Coulter and Chamberlain's *Morphology of the Gymnosperms*, a standard textbook for graduate students of botany. A large number of illustrations, chiefly photographs of gymnosperms in their native habitat, have been added, making it a profusely illustrated volume. The text has been brought up to date, particularly in respect to histological, cytological, and paleontological details. New chapters have been added showing the relation of the gymnosperms to the rest of the plant kingdom and discussing their paleontological history. There is a good index and a bibliography of more than 700 titles.

BOTANY. *Principles and Problems. Third Edition.*

By Edmund W. Sinnott. McGraw-Hill Book Co., New York. $3.50. 9 x 5¾; xix + 525; 1935.

Two characteristic features of Sinnott's textbook are the large proportion of diagrammatic drawings and the "Questions for Thought and Discussion" (782 in all) following each chapter. The third edition has benefited by a thorough revision; a more modern system of phylogeny and classification of the vascular plants has been used, and a chapter on experimental morphology has been added. The "Reference Problems" have finally been omitted, a wise move, especially since there are no bibliographies to assist a student in the profitable use of his time in trying to answer these questions. The quality of the illustrations has been considerably improved. There is a good index.

A TEXT-BOOK OF MYCOLOGY.

By Ernst A. Bessey. P. Blakiston's Son and Co., Philadelphia. $4.00. 8⅜ x 5⅝; xv + 495; 1935.

This textbook is admirably adapted to the needs of second-year botany students. It is intended to provide "an idea of the structure, life history and classification of the more important groups of parasitic fungi." It is not limited to these groups, however, for Bessey's interest in systematic relationships is too strong, and accordingly the Mycetozoa and Plasmodiophorales are included too. It is thoroughly up to date. There is an excellent index and the section entitled "Guide to literature for identification of fungi" has an annotated bibliography covering 73 printed pages. We predict that a great many mycologists will be grateful for this feature.

HISTOLOGICAL CHARACTERS OF FLAX ROOTS IN RELATION TO RESISTANCE TO WILT AND ROOT ROT. *U. S. Department of Agriculture Technical Bulletin No. 458.*

By Lytton W. Boyle. U. S. Government Printing Office, Washington. 5 cents. 9¼ x 6; 19; 1934 (paper).

Three types of flax were selected for this study, according to their reaction to root diseases. These types were respectively resistant, partially resistant, and susceptible. Differences in these three groups were found in the stability of the cortical cell walls of the roots. The most important result was that "(1) two types of diseases, namely wilt and root rot, may occur when flax is grown in soil that has produced this crop at frequent intervals for many years, and (2) that resistance to each of these two types of disease is due to distinctly different characters of the flax plants."

THE SOYA BEAN. *Its History, Cultivation and Uses.*

By Elizabeth Bowdidge. Oxford University Press, New York. $2.00. 7½ x 5; x + 82; 1934.

This little book has been prepared to interest the British farmer in the cultivation and many uses of the soya bean. Experiments in England are in the initial stage but indications are that high protein-yielding varieties can be successfully grown. The writer has drawn largely on

the results of investigations carried on in the United States and Canada but she also includes all important data at present available on experimental work in England. Numerous tables are given in the text but there is neither a bibliography nor an index included in the survey. Sir John T. Davies contributes a foreword.

THE TRANSLOCATION OF SOLUTES IN PLANTS. *A Critical Consideration of Evidence Bearing upon Solute Movement.*

By Otis F. Curtis. McGraw-Hill Book Co., New York. $3.00. 9 x 6; xiii + 273; 1935.

This volume is a valuable addition to the little list of monographs on special topics of plant physiology. Curtis has been an active student of the phenomenon of translocation of nutrient materials in plant tissue for many years and so it is only natural that a considerable portion of the book is devoted to his own work. A valuable feature of the book is the summary that follows each chapter, presenting in brief the principal conclusions that can be drawn from the experiments just discussed. There is an excellent bibliography and an excellent index.

LES CÉRÉALES. *Biologie et Applications.*

By R. Legendre. Armand Colin, Paris. 10.50 francs (paper); 12 francs (cloth). $6\frac{7}{8}$ x $4\frac{1}{2}$; 218; 1935.

Legendre's treatise was written for agricultural experiment station workers and for engineers in charge of the storage of grain. The first half of the book is a consideration of the physiology of the wheat grain. The treatment, though sound, is far from being novel. The last half of the book deals with economic considerations, production statistics, and the principles involved in grain storage. The book has been well assembled from the older, standard sources by dint of patient hack work, but there is not much to recommend it to American agricultural workers.

MANAGEMENT OF AMERICAN FORESTS.

By Donald M. Matthews. McGraw-Hill Book Co., New York. $5.00. 9 x 6; xv + 495; 1935.

A thorough and sound exposition of the economic and financial aspect of forest management. The first part deals with the estimation of yields in relation to site and stocking of growth, the different methods of silviculture, their relative advantages and theoretical expectations under specific conditions. In the second part of the book the problems of capitalization and valuation of forests and production are examined in detail. The author has emphasized throughout the commercial objective of forest management so that this book has both theoretical and practical value.

WACHSTUMSBEEINFLUSSUNG UND LEISTUNGSREGELUNG BEI EINER EINZELLIGEN PFLANZE.

By Max Hahn. Sommer und Schorr, Feuchtwangen. 2.50 marks. $8\frac{1}{4}$ x $5\frac{3}{4}$; 75; 1935 (paper).

Hahn reports experiments on the growth and chemical physiology of *Torula rufa*, a new, pigmented species, which he describes. He was mainly concerned with the influence of filter paper on growth and with the influence of mineral salts. Boron and vanadium stimulated growth, and he notes the effects of several other salts besides the ones usually used for nutrition. There is a bibliography and a table of contents.

THE PLANT WORLD. *Plant Life of Our Earth.*

By C. Stuart Gager. The University Society, New York. 65 cents (paper); $1.00 (cloth); (prices slightly higher on West Coast). [Accompanying "explanatory Reading Outline" 5 cents.] 9 x 6; viii + 136; 1933.

This summary of some of the most interesting phenomena in the plant world is clearly and entertainingly written. The author, after giving a brief account of theories of plant origin, discusses: Clas-

sification, life activities of plants, symbiosis, respiration, photosynthesis, color, pollination, plants and insects, seed dispersal, variation, Darwinism and evolution.

A METHOD FOR DETERMINING THE NUTRIENT NEEDS OF SHADE TREES WITH SPECIAL REFERENCE TO PHOSPHORUS. *Black Rock Forest Papers, Vol. 1, No. 1.*

By Harold L. Mitchell. Black Rock Forest, Cornwall-on-the-Hudson, New York. 11 x 8½; 4; 1935 (paper).

MORPHOLOGY

THE BRAIN *Considered Anatomically, Physiologically and Philosophically. Vol. I. The Cerebrum and Its Parts. Vol. II. The Pituitary Gland, The Cerebellum and the Medulla Oblongata.*

By Emanuel Swedenborg. Edited, Translated, and Annotated by R. L. Tafel. Swedenborg Society, London. 15 shillings per volume. 8½ x 5½; Vol 1, xxxviii + 794; Vol. 2, xvi + 645; 1934.

The Swedenborg Society deserves great credit for reprinting this scholarly work which was first compiled and published in 1882–1887. It includes a translation of Swedenborg's manuscript treatise on the brain, written between 1741 and 1744, and relevant material from his *Œconomia Regni Animalis*, published in 1740 and 1741, and *Regnum Animale*, published in 1744 and 1745, plus commentaries and quotations from the work of other observers prior to 1877 who corroborated the observations and theories set forth by Swedenborg. It should be of interest to medical historians.

DIRECTIONS FOR THE DISSECTION OF THE CAT. *Revised Edition.*

By Robert P. Bigelow. The Macmillan Co., New York. 90 cents. 7⅜ x 5; xi + 65; 1935.

A small laboratory handbook covering the various systems and organs of the cat with instructions for their dissection. The student is not "told all" in the text; some of the facts he must either deduce or find out by collateral reading. Frequent literature citations appear in the text to aid in this reading.

VARIATIONS IN THE CYTOLOGY OF THE BLOOD OF GECKOS (TARENTOLA MAURITANICA) INFECTED WITH HAEMOGREGARINA PLATYDACTYLI, TRYPANOSOMA PLATYDACTYLI, AND PIRHEMOCYTON TARENTOLAE. *University of California Publications in Zoölogy, Volume 41, Number 2.*

By Sherwin F. Wood. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 13 + 2 plates; 1935 (paper).

UNIPOLAR INGRESSION IN TRITURUS TOROSUS: *A Hitherto Undescribed Movement in the Pregastrular Stages of a Urodele. University of California Publications in Zoölogy, Volume 39, Number 15.*

By A. Mandel Schechtman. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 6 + 1 plate; 1934 (paper).

THE DETERMINATION OF THE MEDULLARY PLATE IN TRITURUS TOROSUS (RATHKE). *University of California Publications in Zoology, Volume 39, Number 18.*

By A. Mandel Schechtman. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 16 + 1 plate; 1935 (paper).

PHYSIOLOGY AND PATHOLOGY

A BIBLIOGRAPHY OF THE POEM SYPHILIS SIVE MORBUS GALLICUS *by Girolamo Fracastoro of Verona.*

By Leona Baumgartner and John F. Fulton. Yale University Press, New Haven. $5.00. 9¾ x 6½; 157; 1935 (paper).

In 1493 an epidemic broke out among the troops of Charles VIII of France, then at Naples. Whether it was a new disease, brought back from America by the sailors of Columbus, or an epidemic and virulent form of a disease already present in Europe is still a matter for dispute among medical historians. The epidemic, known at first as the Neapolitan or the French disease, "spread throughout Italy, and in a few years Europe was aflame." In 1530 Fracastoro, an Italian humanist and physician,

published a poem in Latin, *Syphilis sive Morbus Gallicus*, from whose hero, Syphilus, a shepherd whose impiety provokes Apollo to punish him with the disease, the latter was given the name which it now bears. Moreover in this poem, as well as in his later prose treatise *De Contagione*, Fracastoro suggested that the cause of contagious diseases was certain *semina*, capable of self-multiplication, which passed from the infector to the infected. The likeness of this to the modern germ theory of disease led Sudhoff to hail Fracastoro as "the father of modern epidemiology."

Fracastoro's poem has not shared the oblivion which has overwhelmed most of the Latin poetry of the Renaissance. In this scholarly bibliography no fewer than one hundred Latin editions and translations into Italian, English, French, German, Spanish and Portuguese are recorded besides 106 titles of biography and criticism. It is amusing to note that Nahum Tate, whose translation of the Psalms into English verse still holds a place in our hymnals, also made the first complete translation of *Syphilis* into any language. The bibliography also contains an eloquent introduction by Dr. Arnold C. Klebs, a photograph of a bust by Danese Cataneo believed by Ilg and Klebs to represent Fracastoro, reproductions of title-pages, and an adequate index.

PACEMAKERS IN RELATION TO ASPECTS OF BEHAVIOR.

By Hudson Hoagland. The Macmillan Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{5}{8}$; x + 138; 1935.

The Experimental Biology Monographs, of which this is the first volume, "will be biological essays by active workers in the field, in which experimental contributions are synthesized and hypotheses proposed which may stimulate further research. They will not only present factual summaries of recent advances in research work but will also show the broad bases and theoretical implications of such advances."

In this interesting monograph Professor Hoagland makes the acute observation that most behaviorists, in spite of their pretensions to objectivity, know too little of physiology to relate effectively their experimental results to specific physiological mechanisms. His aim in this book is "to show how certain principles of general physiology may be useful in investigations of problems of traditional interest to psychologists, but usually interpreted by them in terms devoid of physicochemical meaning." He finds that an important factor in the kinetics of nerve tissue is the ratio of potassium inside the tissue and in its internal environment. He gives reasons for concluding that the adaptation of cutaneous nerve fibers to repetitive stimuli is due to the accumulation around the nerve fiber of potassium released from neighboring epithelial cells by the stimulus. He finds also that the values of μ, the temperature characteristic of the Arrhenius equation, for a wide variety of vital processes are grouped in definite modes and infers that this implies "the existence of a limited number of catalytic substances involved in the control of master reactions determining rates of a wide variety of physiological processes." However, while some of the modes in his Figure 8 are undoubtedly significant, others may well be merely variations of sampling.

A bibliography of eight pages and author and subject indexes are given.

LES MIGRAINES. *Étude Pathogénique, Clinique et Thérapeutique.*

By Pasteur Vallery-Radot and Jean Hamburger, with the collaboration of P. Blamoutier. Masson et Cie, Paris. 45 francs. $9\frac{1}{4}$ x 6; 231; 1935 (paper).

This is an exhaustive and critical exposition of the symptomatology, pathology, and therapy of migraine. Introduced by a historical summary, the first part contains a detailed description of the order of the symptoms in the more common as well as in the more infrequent forms of this condition. In the second part the authors discuss with extreme clearness the theories and observations on the mechanism and etiological factors presumably involved in the production of migraine. They arrive at the generally accepted conclusion that

while the ophthalmic symptoms may be regarded as due to a cerebral vascular spasm (not necessarily but probably of sympathetic origin), there is no consistent explanation for the headache, vomiting, etc. The authors recognize the importance of the hereditary, humoral and other so-called constitutional factors in favoring the production of this syndrome but emphasize the lack of comprehensive data. They believe migraine may be regarded as due either to endocrine, digestive, hepatic, allergic or neurological disturbances. It is towards these basic pathologic conditions that treatment should be directed. And in the third part they evaluate the different methods of treatment proposed. A fairly complete bibliography at the end of each chapter adds still more to the value of this excellent treatise.

ANESTHÉSIE ET ANALGÉSIE. *Organe Officiel de la Société Française d'Anesthésie et d' Analgésie, Vol. 1, No. 1.*

Editor-in-Chief: Robert Monod, with the assistance of A. Gosset, Léon Binet, E. Desmarest, Émile Forgue, E. Fourneau, Pierre Fredet, A. Hautant, M. Tiffeneau, M. Thalheimer; G. Jacquot, Secretary. Masson et Cie, Paris. 60 francs (France and Colonies); 80 francs (foreign) per volume of 4 numbers. 9¼ x 7; 120; 1935 (paper).

In this new journal will be published the results of original research, and critical reviews of the literature in the field of anesthesia and analgesia, realizing in this way a liaison between the inventors (chemists, physiologists and manufacturers) and the users of anesthetics. In the first number appear the following papers: The present state of the question of anesthesia; The sedative and analgesic actions of piperidinomethyl-benzoidioxand (F. 933) and its near derivatives with animals; Rachidian anesthesia by the use of percaine in surgery on the submesocolic region of the abdomen; The extent of anesthesia in the vertebral column in rachianesthesia, Jones's method; Biological modifications produced in the organism by different methods of anesthesia. There is an "Index Bibliographique" of 11 pages in addition to reviews of thirteen papers published in other journals.

HEALTH *and a Changing Civilisation.*

By E. Obermer. John Lane, London. 3s. 6d. net. 7¼ x 4¾; xv + 171; 1935.

What is the relation of the medical scientist to health, in contradistinction to his usual relation to disease, and what can be done about it? This is the problem presented and discussed in the present volume. The author claims that if a real system of preventive medicine is to be developed the layman, as well as the professional, must put his shoulder to the wheel. One way to meet the problem is, of course, through the further extension and perfection of public health services. The other way, which must work along with the former, is the introduction of a competent, open to all, individual health service. Doctor Obermer visualizes here an institute which he calls the Individual Health Centre; an institution whose organization and functions are developed in his book.

This is an interesting book and, although it is not unusually original, yet it does suggest and strive for an important public enterprise.

INDIVIDUAL HEALTH. *A Technique for the Study of Individual Constitution and Its Application to Health. Volume I, Biochemical Technique.*

By E. Obermer and R. Milton. Chapman and Hall, London. 15 shillings net. 8½ x 5½; xvi + 244 + 12 plates and 4 folding tables; 1935.

This work is written for the "physician of the future" whose primary concern will be "to prevent disease by the promotion of health or maximum functional efficiency." This volume is the first of two outlining a proposed technique of a complete "adaptational survey," the objective of which is:

> to assess the efficiency with which the subject adapts himself to his particular environment, his particular mode of life, diet, exercise, rest, sleep, etc.
>
> It follows for instance therefore that very little, if any, information can be derived from the almost universal practice of carrying out analyses on 24 hour specimens of urine only. When serial quantitative

analyses of the normal constituents of the urine are made and plotted out on a graph . . . these are capable of mirroring fluctuations in the Acid Base balance and Mineral metabolism of the individual which are of the greatest diagnostic interest.

The present volume is confined to detailed descriptions of biochemical techniques which the authors have worked out for serial analyses of blood, ingesta, urine and faeces. It contains numerous illustrations and inserted charts of record forms and floor plans, a bibliography and an index.

SOME NOTABLE EPIDEMICS.

By H. Harold Scott. William Wood & Co., Baltimore. $4.75. 8½ x 5½; xix + 272; 1934.

The purpose of this "anthology" is to show how the many obscure and puzzling problems of British epidemics of the past have been met by investigation. Beginning with the Broad Street Pump outbreak of cholera in 1854 Dr. Scott has chosen those epidemics of enteric fever, diphtheria, scarlet fever and food-poisoning which are of sufficient historical value to have become classic or which illustrate particular points, such as the first employment of chlorine as a means of disinfecting watermains, or water supplies, or the use of pasteurization in cutting short a milk borne epidemic, etc. The survey is of importance to the student of medical history in that material that has not been generally available heretofore is now at his disposal. The student of public health will find that a knowledge of the contents of this book, which so clearly emphasizes the importance of an accumulation of evidence from medical, chemical and sociological sources in any investigation of obscure outbreaks, will be of value in his own work. The volume contains a number of tables and graphs and is documented and indexed.

DIE BEHANDLUNG DER ALTEN UND KRANKEN BEI DEN NATURVÖLKERN.

By John Koty. W. Kohlhammer, Stuttgart. 13.50 marks (paper). 9½ x 6¼; xxxix + 374; 1934 (paper).

The first part of the book is a survey of the treatment of the aged among primitive peoples and early German, Scandinavian, Slavic, Greek, Oriental, etc. cultures. The second part is a critical discussion of the major theories advanced concerning the origin and development of the custom of eliminating from the tribe the aged, diseased, and crippled, either by exposure and neglect or actual killing. None of these theories—evolutionism, nomadism, the religio-magic theory advanced by Lévy-Bruhl, or Freud's hypothesis—does Koty find comprehensive enough. He himself is inclined to believe that these customs are due to a combination of causes, utilitarian and non-altruistic in character, arising from more complicated methods of obtaining food and a living, enmity between tribes, etc.

The book is a scholarly compilation, well documented, and equipped with a thirty-page bibliography and an index. It is designed for the layman interested in social origins, as well as for the specialist.

THE CARE OF THE AGED, THE DYING AND THE DEAD.

By Alfred Worcester. Charles C Thomas, Springfield, Ill. $1.00. 7¼ x 5; vii + 77; 1935 (paper).

In this booklet are reprinted three lectures on the art of medical practice by the Henry K. Oliver Professor of Hygiene at Harvard. In the first, "The Care of the Aged," he gives some sane advice to medical students and young practitioners on the care and treatment of elderly patients. In the second, "The Care of the Dying," he deplores the deterioration of the art of caring for the dying, with the result that there is much less known about it, and admonishes the physician to stand by and give all possible comfort and relief to the patient until the end. The third lecture, "The Care of the Dead," carries the obligations of the physician still further—to aid in the advance of medical science by securing consent for post-mortem examinations; to aid society by breaking down the prejudices against cremation and thus saving the bereaved family from the extortionate prices of the undertakers and avoiding land sequestration for graveyards.

MEDICAL AND SANITARY REPORTS FROM BRITISH COLONIES, PROTECTORATES AND DEPENDENCIES FOR THE YEAR 1930. *Tropical Diseases Bulletin Vol. 29, Supplement.*

Summarized by H. Harold Scott. Bureau of Hygiene and Tropical Diseases, London. 2s. 6d. net. 9¾ x 6⅛; 148; 1932 (paper).

MEDICAL AND SANITARY REPORTS FROM BRITISH COLONIES, PROTECTORATES AND DEPENDENCIES FOR THE YEAR 1931. *Tropical Diseases Bulletin Vol. 30, Supplement.*

Summarized by H. Harold Scott. Bureau of Hygiene and Tropical Diseases, London. 5 shillings net. 9¾ x 6⅛; 238; 1933 (paper).

MEDICAL AND SANITARY REPORTS FROM BRITISH COLONIES, PROTECTORATES AND DEPENDENCIES FOR THE YEAR 1932. *Tropical Diseases Bulletin Vol. 31, Supplement.*

Summarized by H. Harold Scott. Bureau of Hygiene and Tropical Diseases, London. 5 shillings net. 9¾ x 6⅛; 219; 1934 (paper).

These are detailed and useful summaries of the hygienic conditions existing in each of the British Colonies, protectorates and dependencies. Vital statistics, maternity and child welfare, school hygiene, sanitation, water supply, hospital, dispensary and clinical returns and chief diseases treated, are discussed in most instances, and accurate and full data are given.

SYNDROMES MÉTÉOROPATHOLOGIQUES ET INADAPTÉS URBAINS.

By G. Mouriquand and P. Josserand. Masson et Cie, Paris. 16 francs. 7¾ x 5½; 126; 1935 (paper).

A summary of some clinical conditions associated with meteorological changes. Besides the well-known effects of heat and cold the authors describe a more complex condition which they name South Wind syndrome and which is also known as the Föhn syndrome. In infants it is characterized by general irritability, headaches, temperature rises and symptoms of dehydration, and is supposed to occur (in Lyons where the authors have made their observations) during those periods when the wind blows from the south. The authors also discuss what they call "Les inadaptés urbains." These are people living in cities, who, whether definitely ill or not, are inevitably benefited by a change to the country. It is supposedly a constitutional weakness that is inherited. This book being essentially a review of the literature, the subject has received superficial treatment; this fault is compensated by an extensive bibliography of 28 pages.

CELL NUTRITION AND MEDICATION *for Layman, Student and Practitioner. An Introductory Treatise on Phytotherapy and Biochemistry, with a More Advanced Section Dealing with these Sciences in Practice.*

By Eric F. W. Powell. C. W. Daniel Co., London. 7s. 6d. net. 8 x 5¼; 163; 1934.

This volume deals with phytotherapy, a high-toned euphemism for "yarb doctoring," and with a quaint philosophy, which is that "all the substances necessary to heal, restore and overcome diseased conditions are normally found within the organism itself; that the body is a self-curative machine, and that it will maintain its efficiency provided it has a normal supply of all those vital elements peculiar to its own substance."

This reminds us of two lines of a verse about Mary Baker G. Eddy that Reginald, the Office Boy, likes to sing while doing his laboratory chores. It is the 136th of 249 verses of an immortal patriotic song entitled "I am a One Hundred Percent American." The lines referred to are:

> Disease is now laid on the shelf
> And the dead man only kids hisself.

IDEAL HEALTH *or The Laws of Life and Health. Third Edition.*

By Alexander Bryce. William Wood & Co., Baltimore. $2.00. 7¼ x 4¾; xi + 340; 1935.

The third edition of this little text on the preservation of health is very readable for the layman. Health, the author emphasizes, has no necessary relation to strength but is defined as "that condition of the body which prevents the growth of pathogenic micro-organisms, and in its simplest aspect is exemption from disease," and

on the positive side, "a healthy organism is one which can adapt itself and go on adapting itself, both in structure and function to its surroundings." The physiological effects of food, drink, work, rest, air, exercise, cleanliness, clothing, regularity and moderation, are dealt with. There is an interesting chapter on the influence of the mind on the body, and a closing chapter on eugenics.

QUARTERLY BULLETIN OF THE HEALTH ORGANISATION. *Special Number, January 1935. Biological Standardisation.*

League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York. 2s. 6d.; 65 cents. $9\frac{1}{2}$ x $6\frac{1}{4}$; 127; 1935 (paper).

This special number of the *Bulletin* contains the report of the permanent commission on biological standardization and several articles regarding the experiments which were made to arrive at an international standard in the preparation of certain antitoxins and sera. P. Hartley and P. White write on a proposed standard for gas gangrene and toxin (vibrion septique); L. E. Walbrun and C. Reymann, for gas gangrene antitoxin (Edematiens); P. Hartley and W. Smith, for antipneumococcus serum (type I and type II); P. Hartley and M. L. Smith, for staphylococcus antitoxins. In addition there is a report of the conference held in 1932 on the standardization of sex hormones.

A CLASSIFIED BIBLIOGRAPHY ON PSYCHODIETETICS. *Psychological Monographs, Vol. XLVI, No. 2, Whole No. 206.*

By Martin F. Fritz. Psychological Review Co., Princeton, N. J. 75 cents. $9\frac{3}{4}$ x $6\frac{3}{4}$; 53; 1934 (paper).

This bibliography of the relationship between diet and mental phenomena consists of 669 references to articles in English appearing up to the year 1933. These are classified under the headings of pernicious anemia, pellagra, sprue, acrodynia, migraine, epilepsy, appetite, racial vigor and temperament, endurance, intelligence and learning, mental disorders, intestinal toxemia, sense organs, allergy, sex expression, longevity, hypertension, Ménière's disease, beriberi, and general. Besides the titles brief abstracts of the articles are given. This gathering together of results emphasizes the very different conclusions to which different investigators have come.

MODERN MOTHERHOOD. *A Book of Information on Complete Maternity Care: Prenatal—Delivery—Aftercare.*

By Claude E. Heaton. Farrar and Rinehart, New York. $2.00. $7\frac{3}{8}$ x $4\frac{7}{8}$; xxii + 271; 1935.

This is an excellent handbook for the woman interested in the practical physiology of prenatal and postnatal care. Three of the book's chief attributes are its clarity of presentation, its removal of much of the mysticism usually shrouding such matters, and its practicality in suggesting helpful routines.

The modernity of the book can be realized from the following quotation:

> Little need be said about clothing. To read most of the absurd advice given, one would think doctors know very little about what a pregnant woman should wear. Those women who are used to high heels are not going to wear flat ones. . . .

THE CONQUEST OF SUFFERING.

By Ritchie Calder. Methuen and Co., London. 5 shillings net. $7\frac{3}{8}$ x $4\frac{3}{4}$; xvi + 166; 1934.

In this volume the author describes some of the more spectacular developments of medical and physiological science such as "The war against death," "The retreat from the slums," "The battle for babies," etc. The book is well written but seems to lack in places the critical judgment of the scientific man and suggests, as is indeed true, that the author is a competent journalist rather than a medical or biological scientist. There is a foreword by J. B. S. Haldane.

FOOD AND HEALTH.

By Henry C. Sherman. The Macmillan Co., New York. $2.50. $7\frac{3}{4}$ x $5\frac{3}{8}$; x + 296; 1934.

This book discusses all that is most recent in nutritional knowledge. Vitamins are emphasized and their necessity for a healthful diet. The economic value of various food stuffs in relation to their calorific and vitamin content is also discussed at some length. A considerable section in the form of appendices is devoted to tables of caloric content of various foods and to illustrative meals covering three meals a day for six months. The book is intended for the layman and should contain much interesting and easily understood information for the housewife.

MYOSITES *Abcès des Muscles.*

By J. Pergola. Masson et Cie, Paris. 35 francs. 10 x $6\frac{1}{2}$: xiv + 166 + 3 plates; 1935 (paper).

An etiological, anatomical, and pathological study of myositis or abscesses of muscular tissues. Part I treats the acute types of sanguineous origin, Part II the chronic, or syphilitic, tuberculous, and sclerous types. The sclerous type is one of the most difficult to diagnose since its etiology simulates clinically that of malignant neoplasms. The book is illustrated and equipped with a bibliography.

PRÉCIS DE PHYSIQUE MÉDICALE.

By André Strohl. Masson et Cie, Paris. 60 francs (paper); 70 francs (cloth). $7\frac{3}{4}$ x $5\frac{1}{2}$; 723; 1935.

This handbook contains a very clear exposition of the applications of physics to physiologic problems. The first and second parts treat of energy, acoustics and optics in a complete but rather superficial manner. Greater attention is given instead to those phenomena related to light radiation, electricity and radiology. These are described in the third part of the book and the author has recorded in great detail the results of the more recent discoveries and experiments.

LE BILAN MATÉRIEL ET L'ÉNERGÉTIQUE DES SYNTHÈSES BIOLOGIQUES. *Actualités Scientifiques et Industrielles, 214. Exposés de Biologie (Physiologie Cellulaire), I.*

By Hiroshi Tamiya. Hermann et Cie, Paris. 10 francs. 10 x $6\frac{1}{2}$; 45; 1935 (paper).

Tamiya's method of analysis of the respiratory process in fungi is based on a comparison of the CO_2/O_2 ratios obtained by the combustion of the mycelium and the carbohydrate on which it was grown. The booklet is devoted to the mathematical development of this concept and to the application of the results to the data obtained by Terroine and his associates, Algera, and the author.

DIE PHYSIOLOGISCHEN UND PHYSIKALISCHEN GRUNDLAGEN DER HAUTTHERMOMETRIE. *Mit einem Anhang über weitere physiologische Temperaturmessungen.*

By H. Pfleiderer and K. Büttner. Johann Ambrosius Barth, Leipzig. 4.80 marks. $9\frac{1}{4}$ x $6\frac{1}{4}$; viii + 52; 1935 (paper).

A description of techniques and equipment for the measurement of skin temperature, which are simple and inexpensive enough for use in small laboratories, clinics, and field investigations. There is a list of firms from which the necessary apparatus may be purchased, and a bibliography.

THE CIRCULATION OF BLOOD IN AMMOCOETES. *University of California Publications in Zoölogy, Volume 39, Number 16.*

By J. Frank Daniel. University of California Press, Berkeley. 50 cents. 10$\frac{1}{4}$ x $6\frac{1}{2}$; 29; 1934 (paper).

QUARTERLY BULLETIN OF THE HEALTH ORGANISATION. *Volume 3, No. 4.*

League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York. 2s. 6d.; 65 cents. $9\frac{1}{2}$ x $6\frac{1}{4}$; 200; 1934 (paper).

BIOCHEMISTRY

A Textbook of Biochemistry.

Edited by Benjamin Harrow and Carl P. Sherwin. W. B. Saunders Co., Philadelphia. $6.00 net. 9 x 6; 797; 1935.

The thirty chapters of this excellent biochemistry reference book have been written by as many prominent American and British biochemists who are working actively in the fields they review. Although the treatment accorded to their subject matter varies greatly, most of the authors have shown a biological, rather than a medical bias in the selection of their subject matter, laying emphasis on the features that man possesses in common with the lower animals instead of on the clinical or pathological phenomena which are included in texts intended for medical students. There is also an unusual emphasis on the physico-chemical properties of important substances.

Some authors are discursive, as in the chapter on nutrition; others confine themselves to general principles, as is the case in the treatment of enzymes and of cellular physiology; and still others, such as Carl L. A. Schmidt, who writes on the physical chemistry of amino acids and proteins, devote about as much space to tables and graphs as they do to text, and stud the text with equations. In between there is every gradation in technicality. The field of biochemistry is well and capably covered by these chapters, but it would be fairer to call the volume a reference book rather than a text book, in view of this unevenness of treatment, and because the articles are discrete units rather than a planned part of a large-scale logical development of the principles of biochemistry.

Bibliographies, usually of 40 to 200 titles, follow each article. There is an author index by which these citations can be located and an excellent subject index.

Précis de Chimie Biologique Médicale.

By Paul Cristol. Masson et Cie, Paris. 70 francs (paper); 80 francs (cloth). $7\frac{3}{4}$ x $5\frac{1}{2}$; 638; 1935.

A handbook of biochemistry especially adapted to the needs of medical students. It is divided into four parts. The first includes chapters on the chemical constitution of living organisms and on hydrocarbons. The second part concerns the glucides. The author first describes the synthesis due to the action of chlorophyl and then in order outlines the intermediate and final chemical transformations of the glucide molecules which are supposed to take place as a result of metabolism. In this same fashion he treats of the lipoids in the third part and of the proteins in the fourth section. It is an excellent method which is used in most textbooks of physiology but rarely in those on biochemistry. The student is thus able to interpret more intelligently both the physiologic or pathologic processes and the chemical changes. Brief accounts of the latest discoveries are also given in this book but the author has omitted the bibliography.

A History of Food Adulteration and Analysis.

By Frederick A. Filby. George Allen and Unwin, London. 10 shillings net. $8\frac{1}{2}$ x $5\frac{1}{2}$; 269; 1934.

An interesting research into the history of sophistication of food, drugs and drink in England before 1820. The author, a chemist, is especially interested in the development of chemical analysis for the detection of adulteration and it is from this standpoint that he has conducted his investigation. Since in the early period that he covers it was the office of the several guilds to protect the quality and price of their products the author has accordingly subdivided his book and dedicated a chapter each to grocers, bakers, brewers, vintners, and distillers. The information that he has obtained by consulting ancient laws, treatises, etc., reveals remarkable examples of human ingenuity in perpetrating and detecting adulteration.

Biochemical Laboratory Methods *for Students of the Biological Sciences.*

By Clarence A. Morrow. Revised and Rewritten by William M. Sandstrom. John

Wiley and Sons, New York. $3.75 net. 9 x 5⅞; xv + 319; 1935.

This is the second, revised edition of an excellent laboratory manual of plant biochemistry. It was specifically planned to accompany Gortner's *Outlines of Biochemistry* but it has a far wider field of usefulness, in general physiology courses, for instance. A large proportion of the experiments are of a quantitative nature, employing such procedures as the Van Slyke procedures for protein analysis and the Munson and Walker method for sugar determinations. Very satisfactory bibliographies are appended to the directions for each experiment. There are photomicrographs of the osazones of the common sugars. There is an author index and a subject index.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 440. Fermentforschung.* Containing following articles: *Die Ausführung der Abderhalden-Reaktion mittels des Eintauchrefraktometers nach Pregl-de Crinis*, and *Die quantitative Bestimmung der Abwehrfermentwirkung mittels des Interferometers*, by Paul Hirsch; *Pepsin, Trypsin, Chymo-Trypsin*, by John H. Northrop and M. Kunitz; *Ketonaldehydmutase*, by Carl Neuberg and Ernst Simon; *Emulsin*, by Karl Tauböck; *Die Methodik der Dehydrogenasen*, by Torsten Thunberg.

Urban und Schwarzenberg, Berlin. 13.50 marks. 10 x 7; 245; 1935 (paper).

Hirsch describes in the first article the use of the immersion-refractometer method devised by Pregl and de Crinis in determining the Abderhalden pregnancy reaction; in the second, methods of quantitative determination of the action of protective ferments by means of the interferometer, with especial reference to endocrine dysfunction. Northrop and Kunitz discuss the isolation and crystallization of pepsin, trypsin, and chymo-trypsin obtained from the pancreas. The other articles deal respectively with the isolation of the enzyme ketonaldehydemutase and its distribution in plants and animals; emulsin; and a history of methods used for the isolation of the dehydrogenizing enzymes.

GESCHICHTE DER PHYSIOLOGISCHEN CHEMIE. *By Fritz Lieben. Franz Deuticke, Wien.* 20 marks (paper); 23 marks (cloth). 10 x 7; x + 743; 1935.

A comprehensive history of physiological chemistry. Part I, covering 96 pages, treats the subject from the earliest times through the nineteenth century with main reference to the great workers in the field —Lavoisier, Berzelius, Liebig, Arrhenius and others—and arranged in more or less chronological order. Part II carries the investigations to the present time and is arranged by topics. The book is well documented and contains a bibliography of 18 pages, printed in small type with three columns to the page, and a subject index.

ANNUAL REVIEW OF BIOCHEMISTRY. *Volume IV.*

Edited by James M. Luck. Annual Review of Biochemistry, Stanford University P. O., Calif. $5.00. 8¾ x 6; vii + 639; 1935.

To the users of the earlier volumes of this valuable annual review it is only necessary to say that three topics, which have not been treated hitherto in this series, have been included: choline and related compounds, the biochemistry of malignant disease, and plant hormones. In this volume, 27 of the most actively investigated fields of biochemistry have been surveyed by competent specialists who manage to include about half of the previous year's output of papers, noting their bearing on earlier work, and giving citations.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 441. Allgemeine und vergleichende Physiologie (Ergänzung zu Abt. V, Teil 2).* Containing following articles: *Die Bestimmung der Wasserstoffionenkonzentration der Gewebe bei lebenden Tieren mit Hilfe der Kapillarglaselektrode*, by Carl Voegtlin, Herbert Kahler and R. H. Fitch; *Methoden zur Bestimmung des Oxydations-Reduktionspotential*, by Leonor Michaelis;

Methoden zur Untersuchung der Wirkung von α- und β-Strahlen, by Marie Wreschner.

Urban und Schwarzenberg, Berlin. 9 marks. 10 x 7; 158; 1935 (paper).

The first article describes methods and sources of error in the determination of the pH of living animal tissues by means of the capillary glass electrode. The second paper treats methods for the determination of the oxidation-reduction potential, and the third methods for studying the mechanism of action of α and β rays.

LES PROBLÈMES DE LA RADIOGÉOLOGIE. *Actualités Scientifiques et Industrielles 201. Exposés de Géologie, I.*

By W. Vernadsky. Hermann et Cie, Paris. 15 francs. 10 x 6½; 67 + folding table; 1935 (paper).

This is a résumé of the work which has been done by the author and others on the radioactive elements since 1902 when Curie published his first work on the subject. The material was originally presented as two lectures given in 1933 at the University of Paris. A three-page bibliography and a copy of the International Table of radioactive elements are provided.

THE EFFECTS OF METHYLENE BLUE AND OTHER OXIDATION-REDUCTION INDICATORS ON EXPERIMENTAL TUMORS. *University of California Publications in Zoölogy, Volume 39, Number 14.*

By Matilda M. Brooks. University of California Press, Berkeley. 25 cents. 10¼ x 6½; 9; 1934 (paper).

SEX

SEX AND TEMPERAMENT. *In Three Primitive Societies.*

By Margaret Mead. William Morrow and Co., New York. $3.00. 8½ x 5⅝; xxii + 335; 1935.

Miss Mead's problem has been to study in three groups of primitive peoples in New Guinea (the mountain-dwelling Arapesh, the river-dwelling Mundugumor and the lake-dwelling Tchambuli) the "patterning of sex-behaviour from the standpoint of temperament, with the cultural assumption that certain temperamental attitudes are 'naturally' masculine and others naturally' feminine." She found that in two of the three tribes (the Arapesh and the Mundugumor) the idea that temperamental traits of the order of dominance, bravery, aggressiveness, objectivity, malleability, are inalienably associated with one sex (as opposed to the other) is entirely lacking. The Arapesh ideal is the mild responsive man married to the mild responsive woman, the Mundugumor ideal is the violent, aggressive man married to the violent, aggressive woman. In the third tribe quite a different situation exists: the woman is the dominant, impersonal, managing partner, the man the less responsible and emotionally dependent person. From these studies she concludes that we have no basis for regarding temperamental attitudes as sex-linked. In her final chapters she discusses at length the belief that has developed in civilized countries that certain human traits are linked with the opposite sex and the maladjustment that results when parents attempt to condition their offspring to the socially accepted male or female behavior. The general reader, as well as students of ethnology cannot fail to be interested in the contents of this book. The psychiatrist will do well to read it with an open mind. The volume contains an index and a glossary.

MEN AND WOMEN. *The World Journey of a Sexologist.*

By Magnus Hirschfeld. G. P. Putnam's Sons, New York. $4.00. 8½ x 6; xix + 325 + 6 plates; 1935.

An entertaining account of a sexological lecture tour around the world by a distinguished old gentleman, who in common with many another found Germany uncomfortable in its present régime, after having spent a lifetime adding to its scientific fame. Dr. Hirschfeld is a shrewd and wise observer of human behavior, with a broad and tolerant philosophical and historical background. The book rambles somewhat verbosely along in a pleasant

way over a great variety of aspects of the human scene, some of which are passing curious. We are told of the location of brothels near the temples at places of pilgrimage in Japan, to be "eagerly visited by the pilgrims once they have set down their offerings and made their prayers;" of the close relationship between ancestor worship and worship of the phallus; of the great stone elephants at the Ming tombs whereon pregnant women fling small stones—if they stick it will be a boy, if they fall off only a girl; of the ghastly ravages of tuberculosis in China; of the Chinese Jews, there since long before the birth of Christ; of homosexual silk-worm moths; of the quaint Javanese custom of jingling coins at confinement so to lure the baby out more quickly; of the inbreeding in Egypt that has gone on for more than 5000 years.

Dr. A. A. Brill contributes a foreword; the book is sparingly illustrated with photographs, and is well indexed. It will be an interesting and worthy addition to any library of human biology.

Die Folgen der Entmannung Erwachsener. *An der Hand der Kriegserfahrungen dargestellt. Arbeit und Gesundheit, Heft 24.*

By Johannes Lange. Georg Thieme, Leipzig. 5 marks. 9¼ x 6¼; 178; 1934 (paper).

This study is based on 179 males who were totally castrated and 68 partially castrated as results of wounds received during the war, and 63 on whom the operation was performed on account of tuberculous conditions. At the time of study they ranged in age from 35 to 55 years, and the duration of the condition ranged from 11 to 20 years, with a few tuberculous cases under 10 years. A high proportion were found to have undergone physical changes—changes in body proportions, reduced weight and a feminine distribution and limitation of body hair. There was also a high correlation between emasculation and nervous disorders such as hysteria and neurasthenia, but a low correlation in respect to epilepsy and manic-depressive or schizophrenic manifestations. Nearly one half of the book is devoted to detailed case histories.

The Choice of a Mate.

By Anthony M. Ludovici. John Lane, London. 15 shillings net. 8½ x 5½; xxi + 510; 1935.

This 500 page volume is obviously for the mature reader. The author's aim has been to supply authoritative information and guidance in the choice of a mate. To do this he has amassed a vast amount of material which he groups under three general headings. In Part I one finds the following subjects discussed: Obstacles in the way of a sound choice, consanguinity, beauty and ugliness, beauty and health. Part II is devoted to physiognomy, human points and morphology, disease and character, neuroses, positive and negative people, type and character, the female leg and the influence of dress on morphology and temperament, etc. Part III is in general a summing up of what is desirable in a mate, both from the male and the female points of view. The volume is extensively documented and is indexed. Norman Haire contributes an introduction.

Female Sex Perversion. *The Sexually Aberrated Woman As She Is.*

By Maurice Chideckel. Eugenics Publishing Co., New York. $6.00. 9¼ x 6¼; xviii + 331; 1935.

This is a psychiatrical review, couched in non-technical language, of the sexual perversions of the human female. Such subjects as homosexuality, sadism, masochism, exhibitionism, and prostitution, along with others, are discussed in the book. Lay readers may find this book a pretty strong draught. The volume has a certain frankness of attitude about it which will discourage the lascivious minded. The drawings, although interesting, do not seem to be in keeping with the rest of the text.

Sexing Day-Old Chicks. *A Treatise on Sex Detection in Pure- and Cross-Bred Chicks.*

By W. P. Blount. Poultry World, London. 2s. 6d. net. 7¼ x 4⅞; 54; 1934.

This little book sets forth clearly and concisely a method developed over a number

of years by the Japanese in sexing baby chicks. The method is simple, requires no unusual apparatus and can be easily learned by the layman who is willing to sacrifice in the course of his training a certain number of day-old chicks. Speed and accuracy are highly important. Experts, and at present these are mostly Japanese, can sex as many as 600 or even 800 chicks in an hour with a success of 90–95 per cent. Poultry keepers will find this book of great practical value.

BIOMETRY

WAHRSCHEINLICHKEITSLEHRE. *Eine Untersuchung über die logischen und mathematischen Grundlagen der Wahrscheinlichkeitsrechnung.* *By Hans Reichenbach. A. W. Sijthoff's Uitgeversmij, Leiden.* 11.50 H. fl. (paper); 13.50 H. fl. (cloth). 9½ x 6¼; ix + 452; 1935.

Professor Reichenbach, whose *La Philosophie Scientifique* was reviewed in Volume 9, page 258, takes as the epigraph of the present work the following sentence from a letter of Leibniz to Malebranche: "Les mathématiciens ont autant besoin d'être philosophes que les philosophes d'être mathématiciens." His own approach to the difficult problem of the foundations of the theory of probability is along the twin paths of philosophy and mathematics. In the process light is thrown on the significance of *a priori* probabilities and a generalization of symbolic logic is developed in which the two values, truth and falsity, are replaced by a continuous scale of truth values. This makes possible the solution of the problem of induction which since the days of Hume has stood as an unsolved riddle in the path of epistemology. Induction does not give us certainty but it is the only systematic procedure for practical action that we know of.

A blind man, who has lost his way in the mountains, discovers a path with his staff. He knows not whither the path goes, not even that it may not lead to a precipice down which he will plunge. And yet he will follow the path, feeling his way step by step with his staff. For if there is any possibility of his escaping from the wilderness it is by feeling his way along this path. We stand like blind men before the future but we feel our way along a path and we know that if we are to find any route through the future it will be along this path.

PRACTICAL APPLICATIONS OF THE PUNCHED CARD METHOD IN COLLEGES AND UNIVERSITIES

Edited by G. W. Baehne. Columbia University Press, New York. $4.50. 10 x 7; xxii + 442; 1935.

During the United States census of 1880 it became evident that with the rapid increase in population the methods of hand tabulation of the census data then in use would soon break down through sheer unwieldiness. John S. Billings, the distinguished sanitarian and founder of the *Surgeon General's Catalogue* and *Index Medicus*, who was in charge of the collection of vital statistics, suggested to a young engineer in his office, Herman Hollerith, that "There ought to be some mechanical way of doing this job, something on the principle of the Jacquard loom, perhaps, whereby holes on a card regulate the pattern to be woven." Hollerith took up the suggestion and devised the electrical method of tabulation by means of punched cards which is now so widely used by both statistical and accounting offices in many parts of the world.

This book points out specific methods for the utilization of punched card equipment by colleges and universities, both in the administrative tabulations of the registrar's and business offices and in research in various fields done by the statistical method. Among the fields which are specially treated are psychology and educational research, medical research and hospital statistics, legal research, agricultural research (including genetics), anthropology, astronomy, economics, literature (concordances), and social sciences. In two final chapters Professor H. C. Carver describes the use of the automatic multiplying punch and Professor A. E. Brandt that of the progressive digit method in the computation of moments, correlations, analysis of variance and covariance, and curve fitting by least squares. There is an adequate index.

PSYCHOLOGY AND BEHAVIOR

STUDIES IN EXPRESSIVE MOVEMENT.
By Gordon W. Allport and Philip E. Vernon. With a chapter on Matching Sketches of Personality with Script, by Edwin Powers. The Macmillan Co., New York. $3.00. $7\frac{3}{4}$ x $5\frac{1}{4}$; xiii + 269. 1933.

As the authors point out in their introduction,

> Investigations of personality may be focused upon any one of three different levels of phenomena. The first is the level of traits, interests, attitudes, or sentiments considered as composing an "inner" personality; the second is the level of behavior and expression; the third is the level of impression, the perception and interpretation of behavior by another. Since a discovery on one of these levels establishes a presumption that the phenomenon in question has some counterpart on the other levels, a problem which is elusive on one plane may often be more expediently attacked on another. This is the motive and the plan behind the present study. Instead of approaching the difficult problem of consistency or organization in personality through a study of "inner" dispositions—which, of course, can only be known indirectly through tests and scales—we have chosen to refer the problem to the level of expressive movement and there to examine it in a more direct fashion.

The experiments on which the book is based deal with such behavioral characters as speed of reading, counting, walking, drawing and writing, estimation of known sizes, distances, angles and weights, point and grip pressure in drawing and writing, muscular tension, and ratings on voice intensity, speech fluency, amount of movement during conversation and during idleness, freedom and smoothness of movement, tempo of activity, neatness, and forcefulness of personality. These characters, it was found, are neither wholly independent nor completely correlated but fall into several groups, the members of each of which are rather highly correlated with one another but show only small correlations with members of other groups. The second part of the book deals with handwriting and personality. In several experiments under controlled conditions graphological analyses, while by no means perfect in accuracy, showed somewhat greater correspondence with analyses by other methods than was to be expected from chance alone.

> From our results it appears that a man's gesture and handwriting both reflect an essentially stable and constant individual style. His expressive activities seem not to be dissociated and unrelated to one another, but rather to be organized and well-patterned. Furthermore, the evidence indicates that there is congruence between expressive movement and the attitudes, traits, values, and other dispositions of the "inner" personality. Although the question of organization of the personality as a whole is beyond the scope of this volume, it is clear that the foundations for an adequate solution of this important problem cannot be supplied by the anarchic doctrine of specificity but only by the positive and constructive theories of consistency.

There are a bibliography of 206 titles and author and subject indices.

A HANDBOOK OF SOCIAL PSYCHOLOGY.
By W. C. Allee, Gordon W. Allport, Friedrich Alverdes, R. E. Buchanan, Frederic E. Clements, J. F. Dashiell, Erwin A. Esper, Herbert Friedmann, Edwin Deeks Harvey, Melville J. Herskovits, Catharine C. Miles, Walter R. Miles, Gardner Murphy, Lois B. Murphy, O. E. Plath, Thorleif Schjelderup-Ebbe, Victor E. Shelford, Warren S. Thompson, W. D. Wallis, F. L. Wells, Raymond R. Willoughby, Clark Wissler, Ada W. Yerkes, Robert M. Yerkes. Edited by Carl Murchison. Clark University Press, Worcester, Mass. $6.00. 9 x 6; xii + 1195; 1935.

In the preface to this volume the editor remarks: "It is with something akin to despair that one contemplates the piffling, trivial, superficial, damnably unimportant topics that some social scientists investigate with agony and sweat. And at the end of all these centuries, no one knows what is wrong with the world or what is likely to happen to the world." These are harsh words, yet, especially after reading this symposium, one must agree with him. For example, the object of this book is to illustrate the methods and results of investigating human social activities. However, with but few exceptions most of the articles deal either with non-human social behavior or with "abnormal" human behavior and contribute to the study of the main problem very indirectly if at all. Part 1 contains articles on the population behavior of bacteria, social origin and processes among

plants, growth factors in human populations. The second part contains articles on insect and bird societies and mammalian packs and herds. The third part treats of the social history of the Negro, red man, white man and yellow man. The first of these papers on the Negro by M. J. Herskovits deserves special mention. The fourth part deals with language, magic and cognate phenomena, and material culture. The articles on magic by R. R. Willoughby and on material culture by C. C. Wissler are good though somewhat superficial. In the fifth part, on the analysis of some correlates of social phenomena, there is a fine article by G. W. Allport on Attitudes. However, he mentions neither Sumner's *Mores* nor Pareto's *Residues*. The sixth part deals with the social behavior of birds, infrahuman primates, and school children. Here is an excellent paper by J. F. Dashiell on "Experimental studies of the influence of social situations on the behavior of individual human adults."

If one leaves out any consideration of the main purpose of this book, it can be said that almost every one of the papers included is well written, interesting and worth reading.

THE PSYCHO-BIOLOGY OF LANGUAGE. *An Introduction to Dynamic Philology.*

By George K. Zipf. Houghton Mifflin Co., Boston. $3.50. $8\frac{1}{8}$ x $5\frac{1}{2}$; xi + 336; 1935.

A highly interesting book. Unfortunately an adequate conception of its contents can not be given in a limited space. The investigator has by the application of statistical principles studied speech as a natural phenomenon—as a "peculiar form of behavior of a very unusual extant species." He views the stream of speech as a series of communicative gestures. Among the important findings are to be noted the following: the length of a word is closely related to the frequency of its usage—the greater the frequency, the shorter the word; the more complex any speech element is phonetically, the less frequently it occurs; the frequency distribution of words is of a high degree of orderliness, i.e., the most frequent word in any extensive sample of connected English will occur on the average once in approximately every 10 words, the second most frequent word once in every 20 words, the tenth most frequent word once in every 100 words, the *n*th most frequent word in every 10*n* words; if the number of different words occurring once in a given sample is taken as x, the number of different words occurring twice, three times, or n times is according to the inverse square, valid for well over 95 per cent of all the different words used in the sample. His results point quite conclusively to the "existence of a fundamental condition of equilibrium between the form and function of speech-habits, or speech-patterns, in any language." Since words are selected according to their meanings and according to the ideas and feelings which one wishes to convey, Zipf has devoted his research also to the problems of meaning, emotion and mental behavior in general in the light of his results in the stream of speech.

The various chapter headings are: The Form and Behavior of Words; The Form and Behavior of Phonemes; Accent within the Word; The Sentence: Positional and Inflectional Languages; The Stream of Speech and its Relation to the Totality of Behavior. The volume contains tables of statistical data, a section on notes and references, and is indexed.

GESTALT PSYCHOLOGY. *A Survey of Facts and Principles.*

By George W. Hartmann. The Ronald Press Co., New York. $3.50. 8 x $5\frac{3}{8}$; xiii + 325; 1935.

Since the days of the Greeks the problem of the whole and its parts has divided scientists into two camps. Democritus and his followers fixed their attention on the parts, whereas to Aristotle organization was the significant fact. With Dalton the atomic viewpoint triumphed in chemistry and physics and during the greater part of the nineteenth century dominated most of the sciences. With the twentieth century a reaction has taken

place, so that, as Whitehead has said, physics is becoming the study of organisms.

Psychology too has experienced these changes of fashion. Through most of the nineteenth century the associationists, who regarded the mind as a collection of atomic sensations, were the dominant party but towards the end of the century Ward, Stout, von Ehrenfels and Meinong emphasized the organized character of thought. It was not, however, until 1912 that the Gestalt school of psychologists, which has developed the concept of the organization of thought in a more radical way, had its origin in Wertheimer's experiments on apparent movement.

In this book Professor Hartmann aims "to give a sympathetic picture of the Gestalt system from the standpoint of a non-configurationist, although I must confess that an examination of the evidence has left me more favorably disposed toward the theory than I had originally anticipated." He presents the historical background of the doctrine; its physical, physiological and philosophical foundations; the experimental work done by the school, which he feels will prove its most permanent contribution; its practical applications in mental pathology, industry and education; and a résumé of criticisms of Gestalt theory by other schools. Bibliographic footnotes, brief biographies of the more important Gestaltists, a chronology, a glossary and an index are also included.

WISH-HUNTING IN THE UNCONSCIOUS. *An Analysis of Psychoanalysis.*

By Milton Harrington. The Macmillan Co., New York. $2.50. $7\frac{3}{4}$ x $5\frac{1}{8}$; 189; 1934.

Harrington's attack on the principles and practice of psychoanalysis is lively, merciless, and yet fair. The title of his book neatly indicates his chief indictment of Freud's method; it is a process by which the psychoanalyst trains the patient to give him any information he wishes to find, and the technique of interpretation is so unfettered by the rules of logic, or indeed by any other rules, that the analyst can reach any interpretation he really wishes to find. In short, it is a species of faith healing with more than a few analogies with the technique of the old camp-meeting type of revivalist. The first 67 pages are mostly quotations, *in extenso*, of some of the more far-fetched of Freud's dream and symptom interpretations; the next 63 pages are in the best tradition of controversial writing, but the final 43 pages, in which Harrington tries to develop an alternative theory, are pretty disappointing by contrast. Perhaps, in the second volume he promises, Harrington really can explain how he can start with the present-day knowledge of anatomy and physiology and build up a mechanistic system of psychology that will explain the kind of phenomena that Freud is interested in. Skilful as Harrington is as a controversial writer, we suspect he will find that his position is easier to attack than it is to defend.

THE INDIVIDUAL CRIMINAL. *Studies in the Psychogenetics of Crime.*

By Ben Karpman. Nervous and Mental Disease Publishing Co., Washington. $4.50. $8\frac{7}{8}$ x $5\frac{7}{8}$; x + 317 + folding chart; 1935.

The author herein gives an interpretation of the material which he has already published under the heading *Case Studies in the Psychopathology of Crime*, 1933, (noticed in this REVIEW, Volume 8). He includes the life histories in the present volume so that it is unnecessary for the reader to refer to the earlier work. In each case study (there are five altogether) there is given an interpretation of the psychogenetics of the case, a diagnosis, a summary, conclusions, and a discussion of the psychobiology of the individual. In some of the case studies there is an additional section on crime mechanics. Following the studies there is a section on Psychotherapy: Preliminary to the psychotherapy of criminals. The volume contains a chart exhibiting the amnesic attacks of patient five, a general index and an index to criminal jargon.

EYE-MOVEMENT DURING VISUAL IMAGERY. *Comparative Psychology Monographs, Vol. 11, No. 3, Serial No. 53.*

By Edith Totten. Johns Hopkins Press, Baltimore. 75 cents. 10 x 6⅞; 46; 1935 (paper).

Of 105 photographs of the eyes of seven subjects while engaged in visual imagery of assigned objects, 75 showed eye-movements such as would be required in the perception of those objects. Twenty-one were doubtful and nine negative. The author suggests that in those registering no eye-movements some other muscular mechanism may have been substituted, or the muscular change in the eye may have been so slight as not to produce a change in the position of the eye-ball great enough to be caught by the camera. The technique used by the author is described and 56 of the photographs are reproduced. There is a bibliography.

HISTOIRE NATURELLE DE LA CONNAISSANCE CHEZ LE SINGE INFÉRIEUR. *Le Concret. Actualités Scientifiques et Industrielles 215. Exposés de Psychologie Animale, I.*

By Louis Verlaine. Hermann et Cie, Paris. 12 francs. 10 x 6½; 55; 1935 (paper).

This is the first of three studies on the perceptive faculties, concrete and abstract, of Coco, a young *Macacus rhesus*, aged two and a half years. The experiments described in this number include differentiation between squares of paper of different colors, and strips of paper of different lengths or widths, and of various geometrical forms. Further experiments and the conclusions will be given in the other numbers of the series.

THE BEHAVIOR OF SOME YOUNG RAPTORIAL BIRDS. *University of California Publications in Zoölogy, Volume 40, Number 8.*

By E. Lowell Sumner, Jr. University of California Press, Berkeley. 35 cents. 10¼ x 6½; 31; 1934 (paper).

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

LOGIK DER FORSCHUNG. *Zur Erkenntnistheorie der modernen Naturwissenschaft.*

By Karl Popper. Julius Springer, Berlin. 13.50 marks. 8⅝ x 5⅝; vi + 248; 1935 (paper).

It is commonly said that, while such disciplines as mathematics proceed deductively from general propositions to their consequences, the empirical sciences proceed inductively from particular to general propositions. But the logical validity of induction is itself not so clear as to make it a safe foundation for the sciences. If Tom has red hair, and Dick has red hair, and Harry has red hair, am I therefore justified in asserting that all men have red hair? Obviously not. If I multiply my observations and find only red haired men, nevertheless I cannot exclude the possibility that some day I shall discover a black haired man. Popper therefore regards the logic of the empirical sciences as a strictly deductive process. A hypothesis is formed and its consequences are derived deductively. If these consequences are contradicted by experience the hypothesis goes into the waste basket, but if the consequences are not contradicted this does not mean that the hypothesis is proved. It remains in the limbo of provisional hypotheses, subject always to the possibility that some day one of its consequences and therefore the hypothesis itself will be disproved.

The old ideal of science, absolutely certain knowledge, has proved itself an idol. The demand of scientific objectivity leads to the conclusion that every scientific proposition is *provisional*. It can indeed be verified—but every verification is relative to other, likewise provisionally established propositions. Only in our subjective convictions, in our faith can we be "absolutely certain."

With the idol of certainty, even of gradually evolving certainty, falls one of the most difficult obstacles in the path of research; an obstacle not only to boldness in the formulation of a question but often to the rigor and honesty of verification. The desire to maintain one's point betrays a misconception: not the *possession* of knowledge, of irrefutable truths, constitutes the man of science, but the disinterested, incessant *search* for truth.

CAPITALISM *Carries On.*

By Walter B. Pitkin. Whittlesey House, McGraw-Hill Book Co., New York. $1.75. $7\frac{3}{8}$ x 5; xi + 282; 1935.

Mr. Pitkin's thesis is that "Capitalism thrives only where natural resources are abundant, and buyers stand ready to snap up offerings. It can be sustained elsewhere only by an enormously inflated and artificial foreign trade." If the idleness problem can be solved and the nation kept out of war he believes that the old American system can continue. But some form of controlled production must be adopted. An uncontrolled capitalism results in "anarchy, madness and ruin." He has grave doubts, however, as to whether we are able to give capitalism a fair trial. This demands that the "entire middle class must once more be marshalled for research exploration, discovery, invention and creative planning." The highest order of technological skill must be brought into political and social authority. Mr. Pitkin dedicates his book to "The Middle Class on whose survival and prosperity all civilization depends." The volume is without index.

NEW PATHWAYS IN SCIENCE.

By Sir Arthur Eddington. The Macmillan Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{5}{8}$; x + 333; 1935.

In his brilliant and inimitable way Sir Arthur Eddington discusses the more recent advances in the physical sciences. The subject matter of this volume is loosely woven together and includes chapters on the constitution of the atoms, determinism, indeterminacy and quantum theory, the expanding universe, etc. Constantly throughout the book there are expressions of the author's well-known philosophical principles which he summarizes in a final chapter entitled Epilogue. In another chapter he replies to the criticisms of Professor W. T. Stace, C. E. M. Joad, Sir Herbert Samuel and Bertrand Russell. The exposition in general is very clear and the ponderosity of the subject has been successfully lightened by timely and witty digressions. More than a good popular scientific treatise, this is to be regarded as an outstanding example of fine literary style.

SCIENCE AND THE PUBLIC MIND.

By Benjamin C. Gruenberg. McGraw-Hill Book Co., New York. $2.00. 8 x $5\frac{1}{2}$; xiii + 196; 1935.

Says Doctor Gruenberg in the preface, "The book represents the interaction of many minds called upon to think aloud on various aspects of the relationships between that vague entity called 'science' and that perhaps even more vague something known as the 'public.'" The relation of scientific knowledge to the general layman is quite thoroughly developed in the book and the final chapter contains a detailed list of recommendations for making the public more science minded.

THE COST OF BIOLOGICAL BOOKS IN 1935

By JOHN R. MINER

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WHEN the QUARTERLY REVIEW OF BIOLOGY began publication in 1926 the custom was inaugurated of reporting at the end of each volume on the cost of the books which had been reviewed in its columns during the year. The present paper, therefore, marks a decennium of these reports on the cost of biological books. The prices of foreign books have been converted into dollars on the basis of the exchange at the time the books were received.

The total number of pages reviewed in 1935 is 143,199, an increase of 15.6 per cent over 1934 and of 73.4 per cent over 1926, the year in which these tabulations began.

Table 2 shows the price trends of books published in various countries from 1926 to 1935 and the absolute and relative changes in price from 1934 to 1935 and from 1926 to 1935. The average price per page of all the books reviewed in 1935 has changed but little, being 1.032 cents, a decrease of 0.4 per cent from 1934 and of 5.9 per cent from 1926. Books produced by commercial publishing houses in the United States have decreased 3.2 per cent in price from 1934 to 1935 and 19.6 per

TABLE 1

Prices of Biological Books, 1935

ORIGIN	TOTAL PAGES	TOTAL COST	PRICE PER PAGE
			cents
Germany	10,049	$205.02	2.04
British-American	13,253	203.10	1.53
Other countries	8,673	104.47	1.20
United States	80,255	722.45	0.90
France	13,229	114.36	0.86
Great Britain	14,540	122.55	0.84
British Government	605	3.00	0.50
U. S. Government	2,595	2.95	0.11

TABLE 2

Comparison of the Prices of Biological Books from 1926 to 1935

ORIGIN	AVERAGE PRICE PER PAGE										CHANGE + OR − FROM 1934 TO 1935		CHANGE + OR − FROM 1926 TO 1935	
	1926	1927	1928	1929	1930	1931	1932	1933	1934	1935	Absolute	Relative	Absolute	Relative
	cents	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*per cent*	*cents*	*per cent*
British-American	1.55	1.39	1.46	1.90	1.91	2.27	1.48	1.29	1.45	1.53	+0.08	+5.5	−0.02	−1.3
Other countries	1.51	0.78	1.13*	1.68	0.97	1.53	1.02	0.85	0.86	1.20	+0.34	+39.5	−0.31	−20.5
Great Britain	1.28	1.14	1.09	1.29	1.13	1.19	0.89	0.66	0.96	0.84	−0.12	−12.5	−0.44	−34.4
United States	1.12	1.09	1.14	1.14	1.09	1.05	1.00	1.02	0.93	0.90	−0.03	−3.2	−0.22	−19.6
Germany	1.09	1.20	1.48	1.65	1.82	1.75	1.60	1.43	1.89	2.04	+0.15	+7.9	+0.95	+87.2
British Government	—	0.96	1.26	0.39	1.19	1.03	1.45	1.39	0.89	0.50	−0.39	−43.8	−0.46†	−47.9†
France	0.35	0.36	0.45	0.47	0.47	0.69	0.60	0.74	1.00	0.86	−0.14	−14.0	+0.51	+145.7
U. S. Government	0.31	0.24	0.21	0.23	0.30	0.28	0.36	0.17	0.18	0.11	−0.07	−38.9	−0.20	−64.5

* With two special treatises omitted as explained in Vol. 3, p. 601.

† Change from 1927 to 1935.

cent from 1926 to 1935. Books published by the United States Government are not only as usual the least expensive of any of the categories but have actually decreased 38.9 per cent in price since 1934 and 64.5 per cent since 1926.

As to the foreign biological books, those produced and marketed in Great Britain—both government and commercial—and France have decreased in price, expressed in terms of dollars, from 1934 to 1935, while British-American and German books, as well as those produced in countries not separately tabulated, have increased. From 1926 to 1935 prices of foreign books—except for French and German—have decreased. The prices of French books have more than doubled, but their prices in 1926 were so low that the present price is slightly lower than that of commercially published books of American origin. The fear expressed in the first of these reports that "the French publishers will over-compensate in this matter, as the Germans are alleged to have done when their currency became stabilized, and put their prices as much too high as they have been too low in 1926" has, happily, not been substantiated.

German books, on the other hand, are now more than twice as expensive per page as American, French or British. As has been noted in previous reports, the high prices of German scientific books and periodicals have worked great hardship to both libraries and individual scientists. It is, however, reported in *Science* for October 4, 1935 (Vol. 82, p. 327) that a 25 per cent reduction in export price on German books, periodicals and continuations became effective beginning September 9, 1935. This is too late to be reflected in the present report but we hope that next year's report will show the reduction.

The reader should bear in mind that these reports are based on small samples of books in general and, for some countries, on small samples of the biological books published. He should therefore be cautious in applying conclusions drawn from this material to the general domain of book prices.

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